JOURNAL

OF THE

FRANKLIN INSTITUTE

OF THE STATE OF PENNSYLVANIA,

FOR THE PROMOTION OF THE MECHANIC ARTS.

VOL. CX. SEPTEMBER, 1880. No. 3.

THE Franklin Institute is not responsible for the statements and opinions advanced by contributors to the JOURNAL.

EXPERIMENTS ON THE COMPRESSION OF AIR BY THE DIRECT ACTION OF WATER.

By J. P. FRIZELL, C.E.

Some ten years ago, the writer's attention was called to the compressing of air for mechanical purposes by causing it, in the form of minute bubbles, to be carried downward in a current of water and lodged in a receiver by changing the direction of the current from vertical to horizontal.

Such a method appeared to promise important advantages over existing methods of utilizing water power, putting it in a form to be transmitted to distant points and allowing the location of mills and factories to be chosen without reference to the requirements of water wheels.

The application of this method on a scale corresponding to a high degree of efficiency would involve a large expense and its entirely novel and experimental character seemed to render hopeless any attempt to bring it to the notice of capitalists. Nothing was therefore done in the way of its development beyond theoretical researches and some crude and simple experiments till within a couple of years.

Public attention was first called to the subject in a paper published in the JOURNAL OF THE FRANKLIN INSTITUTE for September, 1877, in which the theory of the method was set forth and its probable

advantages pointed out. This had the effect to excite the interest of a prominent New England manufacturer, Gen. J. C. Palfrey of Boston, at whose expense the experiments to be detailed have been mainly conducted.

It was well understood that a practically satisfactory efficiency could not be obtained from a model. With a low velocity, narrow passages and small head, the resistances bear a large proportion to the motive power, and may even absorb it altogether. Theoretical considerations indicate that the efficiency increases with these several elements, till it may in certain conditions reach as high as 80 per cent. or even higher.

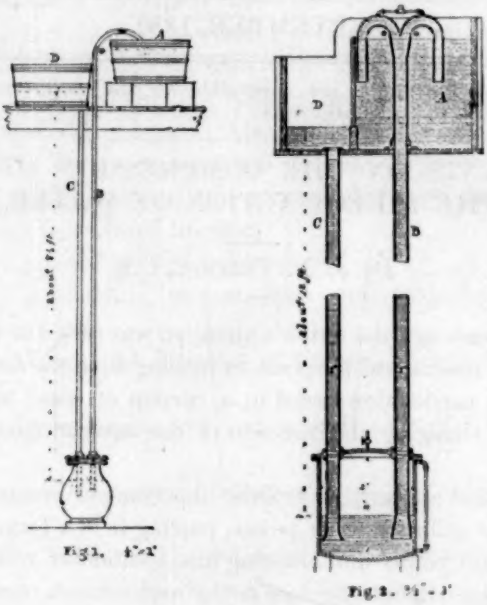


Fig. 1. 1"=1'

Fig. 2. 1"=1'

The apparatus first tried is represented in Fig. 1. Two glass tubes, *B* and *C*, were passed through the cover of a strong glass vessel, the fittings being, as near as possible, air-tight. The tube *B* terminated at the upper end in a siphon, and dipped into a tub, *A*, kept nearly full of water. The tube *C* opened into another tub, *D*. The tube *B* had a small orifice at the summit, closed by a flap valve, for exhausting the air. There was another minute orifice at *o*. The two tubs being filled with water as indicated by the dotted lines, the water, of course, filling the receiver and rising in the tube *B* to the level of the

surface in the tub *D*, the siphon was set in action by placing the finger over the orifice *o*, and exhausting the air by applying the mouth to the summit orifice, then closing the latter. The flow was kept up by dipping the water from the lower to the higher tub. The pressure within the tube at the point *o* being a little less than that of the atmosphere, the air enters there and is carried downward by the current and lodged in the receiver.

This device worked very well as an illustration of the method. The quantity of air collected in the receiver was very small. No attempt at an exact estimate of the useful effect was made. It was very slight, probably not over 5 per cent., certainly not over 10 per cent. of the power expended. The diameter of the tubes was 1¼ inches. The air accumulated under a pressure of 8 or 9 feet of water. The head or difference of level between the two tubs was 8 to 12 inches.

No difficulties not previously foreseen being developed by this experiment, it was decided to try the method on a larger scale. For this purpose the apparatus represented in Fig. 2 was constructed. The receiver was of cast iron, circular, 2 feet diameter, 2 feet high. The pipes *B* and *C* were of iron 3 inches diameter inside. The water was supplied from a wooden tank in two compartments, *A*, *D*, the flow being maintained by pumping the water from the lower to the higher level, with a small centrifugal pump driven by steam, which does not appear in the cut. The pipe *B* terminated, as before, in a siphon, or rather this time, in two siphons, provided at their summits with connections for an air pump to set them in action. The air was admitted, as before, through minute orifices, *o*, *o*, just above the surface of the water in the chamber *A*. A glass gauge on the receiver showed the quantity of air contained in it. The pressure of the air is measured by a column of water equal to the height of the surface in the chamber *D* above that in the receiver.

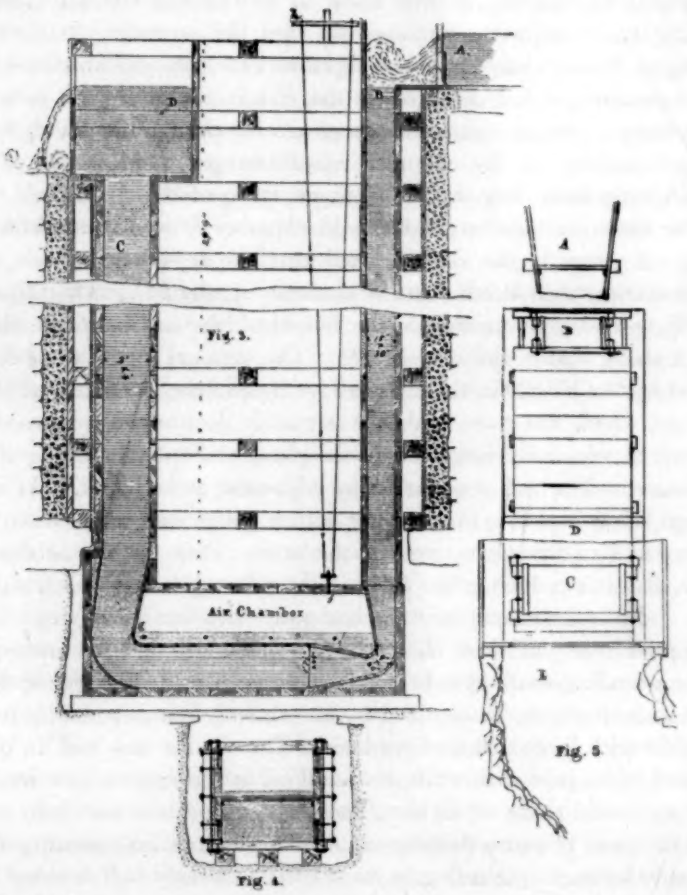
This apparatus was exhibited at the fair of the charitable Mechanic Association in Boston, in 1878. It was afterwards set up in the laboratory of mechanical engineering at the Massachusetts Institute of Technology, and became the subject of critical experiments by Professor Whitaker and members of his class. The quantity of water was inferred from the velocity of the pump. The velocity could also be observed, but not very closely, in the descending shaft, which, near the bottom, was of glass. From the best data attainable, Mr. Whitaker computed the efficiency at 26½ per cent; that is, the power repre-

sented by the air after compression is but 26½ per cent. of that represented by the water employed in compressing it. The head in this experiment was about 25 inches. This result, though at once accepted by the unthinking as a failure of the method, was so entirely in accordance with the true theory of the subject that it strengthened our confidence and induced us to make a trial upon a larger scale. In the meantime I had been called by business to St. Paul, Minn.

The application of this method on any greater than the laboratory scale requires conditions very rarely met with, viz.: a deep pit, free from water, in the immediate vicinity of a small waterfall. These conditions happened to be presented at the Falls of St. Anthony, on the Mississippi river a few miles above St. Paul. A vertical shaft was sunk by the United States Engineer Department, several years since, in the work for the preservation of the falls, and had never been refilled. It was about 36 feet deep and was drained by a pipe opening into the river below the falls. Its clear dimensions, inside the timbering, were about 6 × 14 feet. It occupied a narrow island between two mill ponds, one of which was maintained some 8 or 10 feet higher than the other.

Permission to occupy this shaft for the purpose in question was readily given by the officer in charge of works in this district, C. J. Allen, Captain of Engineers, and Dr. S. H. Chute, one of the managers of the St. Anthony Water Power Company, kindly gave permission to use the water and make the necessary excavation for races. The apparatus, except the raceways, is shown in Figs. 3, 4 and 5. Figure 3 is a vertical longitudinal section; Fig. 4 a cross section through the air chamber, and Fig. 5 a plan. The apparatus consists of a strong wooden tank at the bottom of the pit, with two vertical shafts or channels rising to the surface of the ground. The shaft for the descent of the water, in which a considerable velocity is required, is 15 × 30 inches. The ascending shaft, whose function is to return the water to the surface with as little loss of head as possible, is 24 × 48 inches. The upper part of the tank between the vertical passages is occupied by the air chamber which is lined with zinc and is furnished with a cock, *f*, operated from above, as shown, for the escape of air. The air chamber is separated from the passage below it by a partition of boards laid with openings which allow the air to pass freely into the chamber. It will be noticed that the descending shaft, at the bottom, is contracted strongly in the plane of the section, though it expands

somewhat in the direction at right angles thereto. It was apprehended that the air might tend to aggregate in masses as soon as it departed from the perpendicular direction. This disposition was intended to quicken the velocity a little at that point and make the change of



direction as abrupt as possible. This involved some loss of head, more than was necessary, as I am inclined to think. The flaring of the passage, after assuming the horizontal direction, was designed to utilize the *vis viva* lost by the water in passing from the higher to the lower velocity. The dimensions of the pit did not admit of so long a horizontal passage as was desirable. A glass gauge was first applied

to the air chamber, as in the arrangement already described. The drenching to which it would subject an observer, however, decided me to abandon this, and adopt an arrangement bringing all the essential facts of the process under the eye of a single observer above ground. An opening, *g*, was made in the side of the air chamber just 21 inches below the top, opening into the ascending shaft. The filling of the air chamber to the depth of 21 inches was announced by the appearance of large bubbles in the chamber *D*, followed in a few seconds by a violent commotion therein. By raising the lever, *h*, the air accumulated in the chamber was discharged, the cessation of the loud hissing indicating the complete emptying of the chamber.

The water was discharged from the chamber *D* over a weir 5·56 feet long. A gauge in this chamber indicated the depth on the weir, and, in connection with another in the chamber *A*, the fall. The fall, it is hardly necessary to observe, is the height of the surface in the chamber *A* above that in the chamber *D*. The pressure of the air is determined by the height in the latter above that in the air chamber. The leakage, which was considerable, was readily determined by observing the rate at which the water fell in the chamber *D* after shutting off its admission. The fall was varied by adjustable weir plank. At each change of weir plank, the reading of the gauge was taken when the water was exactly at the crest of the weir. This reading subtracted from that taken during the flow of the water gave the depth on the weir. No microscopic nicety, but only substantial accuracy was attempted in any of these observations. The tank and the lower part of the ascending shaft were of four inch pine plank. The rest of three and two inch plank, all confined by an immense number of screw and lag bolts with large cast iron washers. The timber was put in quite dry and joints paid with white lead and oil in the expectation that the swelling would make all tight. This expectation was not fully realized, the great pressure starting some of the joints and causing considerable leakage, amounting in some cases to nearly half a cubic foot per second. This was, of course, added to the quantity passing the weir.

As originally constructed, the upper part of the shaft was provided with a siphon, as in previous arrangements. Meeting with great difficulty in exhausting the air, I decided to take it out and rebuild it on a different plan, but in the meantime took occasion to make an experiment which I had contemplated from the first, viz., to ascertain whether the introduction of the air could not be equally well accomplished by

giving the water a slight fall at its entrance. The readiness with which water in violent agitation breaks into foam is familiar to all, as well as the fact that foam is nothing more than water impregnated with minute air bubbles.

The method adopted was the one indicated in the sketch, viz., allowing the water to fall into the descending shaft over a barrier of stop-plank. The complete success of the experiment made it apparent that the siphon might henceforth be dismissed from the method, although it would undoubtedly have some advantage as regards efficiency, especially with a low head.

In the theory of the subject formerly presented I assumed 12 inches per second as the velocity with which bubbles of air rise in still water. This was founded on experiments with bubbles formed by air escaping through orifices. In the process above described the air appears to be broken up into finer bubbles, which do not rise so fast. I found that a velocity of 0·75 ft. per sec. in the descending shaft brought an appreciable quantity of air into the chamber; 0·86 ft. brought it in freely.

The mode of conducting experiments on compression was as follows: The water being turned on and flowing in full volume over the weir, the air chamber being empty, the escape valve was closed by the lever, *h*, and at the same instant a stop watch was started. The gauges in *A* and *D* were observed and recorded; then the eye was fixed on the surface in *D*. On the appearance of large air bubbles here, the watch was stopped. It must be observed that when the velocity in the descending shaft much exceeded three feet per second large quantities of air were carried past the air chamber, and rose in the ascending shaft. There was no danger, however, of confounding these bubbles with those announcing the filling of the air chamber, the latter being larger and immediately followed by a great commotion in the basin *D*.

The passage of air past the air chamber disposes of one difficulty that has been predicted of this method, viz., that the air would tend to aggregate in large masses. No tendency of this kind was observable in the air coming up the ascending shaft, though it had traversed a distance of over 70 feet. It reached the surface in fine bubbles, very nearly uniform in size.

The capacity of the air chamber, filled to a depth of 21 inches, was 71·19 cubic feet. The following is an example of the computations:

Take experiment No. 1. Fall, 4·07 feet; depth on weir, 0·63 ft.;

quantity of water, $1{\cdot}665 \times (5{\cdot}56 - 0{\cdot}2 \times 0{\cdot}63) +$ (leakage) $0{\cdot}41 =$ 9·45 cubic feet per second.

Pressure of air, ... 27·58 ft. water.

Power of 1 cub. ft. air—Admission, $27{\cdot}58 \times 62{\cdot}3 = 1780{\cdot}5$ ft. lbs.

" " " —Expansion $= \frac{1780{\cdot}5}{2} \times \frac{27{\cdot}58}{34} = 747{\cdot}8$ " "

Total, . . 2528·3 " "

Time, from closing of escape valve to appearance of bubbles, 2 m. 48 s.

Velocity of water in ascending shaft, $\frac{9{\cdot}45}{8} = 1{\cdot}18$ do. of air, 2·18 ft. per second.

Time of rising of air bubbles, $\frac{27{\cdot}58}{2{\cdot}18} = 13$ seconds.

Time of filling of air chamber = 2 m. 48 s. — 13 s. = 2 m. 35 s. = 2·583 minutes.

Quantity of air per minute $= \frac{71{\cdot}19}{2{\cdot}583} = 27{\cdot}60$ cubic feet.

Power of air per minute $= 2528{\cdot}3 \times 27{\cdot}60 = 69781{\cdot}$ ft. lbs.

" " water " $= 4{\cdot}07 \times 9{\cdot}45 \times 60 \times 62{\cdot}3 = 143780$ ft. lbs.

Percentage of useful effect $= \frac{69781}{143780} = 48{\cdot}5$ per cent.

No account was taken of the leakage of air, which was slight. The assumption that the mean pressure during expansion is half the initial pressure rather over-estimates the power of the air. On the other hand, taking an atmosphere at 34 feet water (its value at the sea level) leads to a greater error in the opposite direction. The bottom of the pit is about 750 feet above the sea level.

A large number of experiments were made before discovering the most advantageous conditions of working. Table I contains all the experiments covered by the dates therein given.

The best efficiency is 52 per cent., being about double the best result obtained with the apparatus, Fig. 2. This percentage would, of course, be entirely unsatisfactory in a practical motor; but an air-compressing system on a practical scale would exceed this model, in dimensions, in greater ratio than this exceeds the apparatus represented in Fig. 2. The object of these experiments is to ascertain what could be expected from such a system. Let us first see how these results accord with the theory of the subject.

Table I.

Results of Experiments in Compressing Air by causing it to mix with a Descending Current of Water.

No. of expt.	Date.—1880.	Head.—Feet.	Pressure on air in chamber, in feet of water.	Quantity of water.—Cub. feet per sec.	Quantity of air carried into chamber.—Cub. feet per minute.	Power due to one cub. foot of air.—Ft. lbs.	Total power of air for one minute.—Ft. lbs.	Total power of water for one minute—Ft. lbs	Percentage of useful effect.	Velocity in the descending shaft.—Feet per sec.
1	May 30	4·07	28·58	9·45	27·60	2528·3	69781	143780	48·5	3·02
2	" "	3·24	29·43	9·89	18·59	2627·4	48843	119780	40·8	3·16
3	" "	2·62	30·08	9·45	14·47	2703·2	39115	92549	42·3	3·02
4	" 31	2·54	29·91	5·92	8·53	2683·	22877	56207	40·4	1·89
5	" "	0·98	.....	14·03	0·00	.....	.....	.....	.....	4·49
6	" "	4·82	27·68	10·34	31·40	2426·3	76193	186297	40·9	3·31
7	" "	4·70	27·75	11·89	38·07	2434·	92668	208890	44·3	3·80
8	" "	4·77	27·75	11·89	35·95	2434·	87522	212000	41·3	3·80
9	" "	4·81	27·72	11·22	42·12	2430·7	102391	201733	50·7	3·59
10	" "	4·87	27·68	10·34	36·89	2427·2	89529	188230	47·6	3·31
11	" "	4·94	27·58	8·23	27·31	2415·2	65951	151973	43·4	2·63
12	June 2	4·78	27·64	9·46	34·90	2422·	84519	169040	50·0	3·03
13	" "	4·79	27·64	9·46	36·41	2422·	88200	169381	52·1	3·03
14	" "	5·01	27·47	6·10	21·42	2402·7	51476	114237	45·0	1·95
15	" "	5·02	27·46	5·92	21·44	2401·9	51503	111090	46·4	1·89

The losses of power, or the losses of head, which are the same thing, the head being taken to represent the power in any given experiment, are of three kinds: First, the loss at the entrance, or the fall required to impregnate the air with bubbles. This did not admit of very close measurement, but, as near as could be estimated, did not differ materially from one foot in all the experiments. Second, the resistances to movement. In experiment No. 5, with no air passing, a velocity of 4·49 feet per second in the descending shaft corresponds to a head of 0·98 feet wholly absorbed in resistances to movement. This gives the means of finding the resistance to movement for any other velocity, such resistance being represented by a head which is proportional to the square of the velocity. Third, a certain loss is occasioned by the fact that the air does not move downward with the same velocity as the water. The air bubbles, as I find, would rise in still water at the rate of 0·75 foot per second. When the water, therefore, is moving downward at the rate of 3 feet per second the air goes only 2·25. There is, then, a loss of power of $\frac{0{\cdot}75}{3{\cdot}00} = 25$ per cent. of the head

employed in compressing the air. Let us now apply these principles to computing the head in experiment 1.

Head lost at entrance, ... 1·000 feet.

" " in resistance to movement $= 0{\cdot}98 \left(\frac{3{\cdot}02}{4{\cdot}49}\right)^2 = 0{\cdot}443$ "

" " slip $= (4{\cdot}07 - 1{\cdot}443) \times \frac{0{\cdot}75}{3{\cdot}02}$ . . . $= 0{\cdot}653$ "

" expended usefully $= 4{\cdot}07 \times 48{\cdot}5$. $= 1{\cdot}974$ "

Total, ... 4·070 "

TABLE II.

Expt.	Head.		Expt.	Head.	
No.	Observed.	Computed.	No.	Observed.	Computed.
1	4·07	4·07	9	4·81	4·73
2	3·24	3·22	10	4·87	4·61
3	2·62	2·84	11	4·94	4·51
4	2·54	2·74	12	4·78	4·66
6	4·82	4·25	13	4·79	4·77
7	4·70	4·38	14	5·01	4·92
8	4·77	4·28	15	5·02	5·03

Table II contains the results of this calculation applied to all the experiments. The computed head falls short of the observed head in the experiments with velocities exceeding 3 feet per second, as was to be expected, since in these cases large quantities of air were carried past the chamber, involving resistances not taken account of in the computation. Moreover, in these cases, air counting as water, the tendency is to over-estimate the quantity passing the weir. One thing is especially to be noted. In the experiments giving the best results the agreement is quite satisfactory. We may, therefore, I apprehend, apply this method, with a good degree of confidence, to a system of practical size, understanding always that it is working to its best advantage.

For a system of practical size, we may fix the following dimensions: Descending shaft, 10 feet diameter, making the mean radius six times that of the model. Ascending shaft in same proportion. Depth, 120 feet, corresponding to a pressure of about 50 pounds per square inch, making the journey of the water about four times that in experiment 5. Fall, 15 feet. Velocity in descending shaft, 6 feet per second. It would not be advisable to make such a system an exact copy of the model which, owing to the limited space, could not have the form best calculated to avoid loss of head.

The loss of head in experiment 5 was 0·98 foot, with a velocity of 4·49 feet per second.

The loss in the straight passages, by the best formulas, could not have exceeded . . . . . 0·200 foot.

The loss due to bends, chiefly the bend at the bottom of the descending shaft, which the results show to be more abrupt than necessary, was . . 0·250 "

A loss also occurs at the entrance,

represented by $\left\{\left(\frac{1}{0{\cdot}68}\right)^2-1\right\}\left(\frac{4{\cdot}49}{2g}\right)^2$. . . = 0·365 "

0·68 being the coefficient of contraction of the water at its entrance to the descending shaft. The remainder, 0·165 "

0·980 "

is a part of the head due to the initial velocity which, it is evident, was not wholly suppressed, the enlargement of the channel under the air chamber being a little too abrupt. This is evident, also, from the readiness with which air was carried past the chamber, implying commotion in the water, which retarded the elimination of the air.

The item 0·365 might have been wholly suppressed by suitably rounding the entrance to the descending shaft. We will therefore reject it in applying the results to the proposed system.

The frictional head proper (0·200) is directly proportional to the square of the velocity, directly as the distance traversed by the water, and inversely as the mean radius of the channel. The other items of head (0·250+0·165=0·415) are directly as the square of the velocity. The losses in the proposed system of channels will therefore be as follows:

Frictional head, $0{\cdot}200 \times \left(\frac{6}{4{\cdot}49}\right)^2 \times 4 \times \frac{1}{8} = 0{\cdot}238$ foot.

Other losses, $0{\cdot}415 \times \left(\frac{6}{4{\cdot}49}\right)^2 \quad . \quad = 0{\cdot}741$ "

Head expended in reducing the water to foam, 1·000 "

Slip $(15{\cdot} - 1{\cdot}979)$, $\frac{0{\cdot}75}{6{\cdot}0} \quad . \quad . \quad = 1{\cdot}628$ "

Total losses, . . . 3·607 "

$$\text{Efficiency} = \frac{15{\cdot} - 3{\cdot}607}{15{\cdot}} = 76 \text{ per cent.}$$

Similarly, with a head of 30 feet, reducing the air to a pressure of 100 pounds per square inch, the efficiency would be 81 per cent.

St. Paul, July, 1880.

Stability of Oxygenated Water. — Berthelot experimented upon a liquid containing 3·85 grammes of oxygen per litre (225 grains per gallon), to which ·15 gramme (·48 oz.) of sulphuric acid had been added. When exposed to a temperature ranging between 10° and 15°C. (50° and 59°F.), the decomposition continued, during the first month, in a ratio nearly proportional to the time. Afterwards the reaction gradually diminished, and some samples which were prepared in December, 1877, still include measurable quantities of oxygenated water. When the oxygenated water is pure, or very concentrated, the decomposition at first is much more rapid than would be indicated by a simple proportionality to the time. Then comes a certain consecutive period when such proportionality exists; finally, the reaction diminishes nearly as if following a curve with an asymptote. Similar relations are found in the study of ozone, and probably in every exothermic decomposition which is slowly effected in a homogeneous medium. The velocity of the transformation varies in an extraordinary manner with the presence of foreign substances in the liquid. For example, a liquid enclosing 1·66 grammes of active oxygen per per litre, acidified by ·009 gramme of chlorhydric acid, was decomposed with only one-fifth of the rapidity of that which contained the sulphuric acid.—*Comptes Rendus.* C.

EXPERIMENTS ON THE STRENGTH OF YELLOW PINE.

By Prof. R. H. THURSTON.

[Presented to the American Association for Advancement of Science, Aug., 1880.]

In a paper read at the Saratoga meeting of the American Association for the Advancement of Science the writer presented the result of a series of experiments on the strength of timber, in which were given several unusual figures.*

To determine how far these results were due to peculiarities of the selected samples supplied from the Navy Yard, and to determine to what extent size affects the resistance, a more extended series of transverse tests were made, and the results of experiments upon yellow pine, of the ordinary market qualities and of various dimensions, are now presented below, as determined in the Mechanical Laboratory of the Stevens Institute of Technology.

In the paper referred to, the modulus of elasticity was given for yellow pine as an extraordinarily high figure. It will be observed that the best wood here described gives also very high values of E, and a comparison of the pieces of the first with these test specimens shows the selected Navy Yard specimen to have been of better material than either of the latter.

Samples marked F_1, F_2, F_3, were from the same plank—a piece of yellow pine cut in Georgia, April, 1879, and tested after several months of seasoning, when it had become thoroughly dry. The three specimens were considered good material. F_3 was not straight-grained and broke obliquely, giving a much lower modulus of elasticity, as well as of mixture, than its companion specimens.

Samples B_1, B_2, B_3, were cut from a stick ten inches square in section, which had been lying under cover, seasoning, nine or ten years.

Numbers 1 to 12, inclusive, were small sticks sawn out of the middle of a plank, originally four inches thick, one foot wide, and twenty-four feet long. A stick was first cut three inches square and twenty-four feet long, which was then cut into strips of varying smaller dimen-

* Among these were several due to printers' errors, which, owing to the illness of the writer, were not corrected in proof. The tenacity of yellow pine, for example, was made 2070·2 when it should have been given at 20·702.

sions. The wood was selected from lumber yard stock, and was considered to be fairly representative of average timber. It was cut in Florida, in October, 1879, and reached Hoboken in January, 1880.

Specimens 1 to 8, inclusive, were too green for use in construction; Nos. 9, 10 and 11 were kiln-dried 56 hours, at a temperature of 130° Fahr., No. 12 was dried 12 hours, at 210° Fahr., at which temperature the pitch exuded from the wood quite freely. Still another specimen, not here recorded, was heated 1½ hours at 420°F., and was somewhat charred. Under test it gave a modulus of rupture, $R = 9000$.

In the table containing a *resumé* of results is also given the figures obtained in the earlier experiments on selected Navy Yard material, marked J and those of a sample, K, tested by another observer.*

Specimens A, A_1, were picked up in a workshop, and used simply to determine specific gravity; they were probably not the best Southern long-leaved pine, such as it was intended that the samples tested should be.

Calculating $E = \frac{W l^3}{4 A b d^3}$ for heavy loads and considerable deflections, the following figures were obtained for this *pseudo*-modulus:

TABLE II.—*Values of E at one-third to one-half Total Load.*

No.	*E.*	No.	*E.*	No.	*E.*
F_2	2,136,000	1	2,055,000	7	.....
F_3	1,689,000	2	1,776,000	8	2,086,000
B_1	2,394,000	3	2,032,000	9	2,251,000
B_2	2,334,000	4	.....	10	2,869,000
B_3	2,489,000	5	2,272,000	11	2,574,000
		6	2,231,000	12	2,380,000
				Average.....	2,098,000

These figures differ little from those obtained by calculation from the smaller deflections and loads. It was observed that this modulus sometimes decreases with increase of spring, and sometimes increases. As a general rule, the maximum figure was given at one-third, rarely at one-half, and about as often at the beginning of the test. The latter case occurred most frequently with the unseasoned timber.

* *Van Nostrand's Magazine*, Feb., 1880, p. 166.

The following table contains the values of the Moduli of Rupture and of Elasticity deduced from the whole series of experiments:

TABLE I.—*Strength of Yellow Pine Timber.*

Condition.	No. of Test Piece.	Dimensions. l = distance between supports.			$R = \frac{3}{2}\frac{W\ l}{b\ d^2}$	$E = \frac{W\ l^3}{4\Delta b d^3}$	Weight per cub. ft.	Spec. Grav.
		l ins.	b ins.	d ins.			Pounds.	
Very dry.	F_1	54	2·939	2·946	12,700	.....	41·03	0·66
	F_2	54	2·972	3·008	12,050	1,962,000	45·72	0·73
	F_3	54	3·016	3·000	9,850	1,825,000	40·27	0·64
Old and thoroughly seasoned.	B_1	54	3·046	3·036	13,850	2,178,000	48·89	0·78
	B_2	54	3·016	3·050	13,710	1,949,000	47·14	·75
	B_3	54	3·012	3·017	14,780	2,193,000	46·38	2·74
Too green for use.	1	40	1·285	1·315	12,150	2,171,000	52·85	0·84
	2	40	1·285	1·295	12,530	1,796,000	62·85	1·006
	3	40	1·285	1·295	10,810	1,953,000	.....	.....
	4	40	1·270	1·287	11,350	....	.....	.....
	5	54	2·930	2·930	10,790	2,427,000	.....	.....
	6	54	2·925	2·925	10,940	2,231,000	.....	.....
	7	24	3·06	0·70	12,000	.....	.....	.....
	8	24½	3·032	0·675	.....	2·279,000	.....	.....
	8a	40	3·032	0·675	13,240	2·086,000	.....	.....
Kiln-dried 56 hours at 130°F.	9	54	2·885	2·875	12,100	.....	.....	.....
	10	40	1·240	1·275	17,260	2,564,000	.....	.....
	11	40	1·244	1·274	14,560	2,543,000	54·845	0·88
12 hs. at 210°F.	12	40	1·229	1·242	13,870	2,489,000	51·857	0·83
	K	40	1·23	1·23	14,654	1,892,510	37·72	0·60
	J	54	3·0	3·0	16,740	3,534,727	47·95	0·77
	A	.....	.....	.....	....	.....	39·64	0·63
	A_1	.....	.....	.....	.....	.....	39·50	0·63
Sapwood from	F_3	.....	.....	.....	.....	.....	35·96	0·57
Average...	.....	.....	.....	.....	.....	2,239,000	.....	.....

On the whole it may apparently be taken as practically true that the deflection is very nearly proportional to the load, far beyond customary limits of strain, and that the modulus is consequently very nearly constant for all moderate deflections.

Thus, from the test of B_1 we obtained

TABLE III.

W.	*E.*	*W.*	*E.*	*W.*	*E.*
50	2,178,000	450	2,288,000	1050	2,363,000
150	2,249,000	650	2,327,000	1250	2,381,000
250	2,259,000	850	2,350,000	1450	2,394,000
				4750	Broke.
				Average.....	2,309,888

When light loads, as one-fourth or even one-eighth the maximum, were imposed for a considerable time, as ten or twenty minutes, the deflection gradually and constantly increased, and on removal of the weight steadily decreased, returning nearly to the original set. Heavy loads, long applied, produced fracture of pieces, the companions to which resisted considerably more when the load was steadily increased up to the moment of fracture. The maximum permanent load was apparently something less than one-third and probably greater than one-half the maximum load which could be sustained under ordinary test.

In concluding this series of experiments upon pine timber, a set of four yellow pine beams of large section was tested to determine the value of the Modulus of Elasticity in large timbers, such are used in heavy work, and also to ascertain whether experiments upon small sticks, such as are usually tested, may be relied upon to give correct values of this coefficient.

The following figures were obtained:

TABLE IV.—*Elasticity of Yellow Pine Timber.*

Size. Inches.			Weight. Lbs. per	Spec.	Max. load.	Max. deflection.	$E = \frac{P^1 l^3}{4 \Delta b d^3}$
b.	*d.*	*l.*	cu. ft.	Grav.	P	Δ	Average.
6·187	8·5	144	40·13	0·64	5460	0·7652	110,247 (1)
6·75	8·	200	41·2	0·66	5813	3·8946	895,629
6·875	8·25	150 to 200	.....	.....	1850 to 5900	0·1875 to 2·29	1,768,070 (2)
6·75	8·375	480	45·27	0·72	650	3·234	.....
					Average of all experiments.....		1,387,156

(1) Average of trials at $l = 150$ to 200.
(2) Average of trials at $l = 150$ to 200, and beams in different positions.

In this table b, d and l are the dimensions of the cross-section of the beam and the length between supports. In some cases a beam is credited with an average value of E, it having been tested first on its edge and then on its side. Column l gives the distance between the supports. Wt is the weight of the wood; P is the maximum load in pounds; Δ, the maximum deflection in inches; and E is the coefficient of elasticity.

Nos. 1 and 2 of this lot were of poor quality; the latter, particularly, was somewhat shaky, yet it fairly represents much of the timber which finds its way into important constructions.

The latest work on the strength of timbers which has been consulted by the writer* gives the following values for $E_1 = \frac{W\,l^3}{16\,\Delta\,b\,d^3}$ from which we obtain the proper moduli, $E = \frac{W\,l^3}{4\,\Delta\,b\,d^3}$, as given beside them.

TABLE V.—*Strength and Elasticity of Pine Timber.*

Wood.	Wt.	Spec. Grav.	$R = \frac{3}{2}\frac{l\,W}{b\,d^2}$†	$R = \frac{l\,W}{4\,b\,d^2}$*	$E = \frac{W\,l^3}{16\,b\,\Delta\,d^3}$*	$E = \frac{W\,l^3}{4\,\Delta\,b\,d^3}$†
Pitch Pine	39·37	0·63	14,088	2348	755,240	3,020,960
Dantzic Fir	36·37	0·58	13,806	2301	579,190	2,310,760
Can. Spruce	30·25	0·48	10,544	1759	771,800	3,087,200
" R. Pine	34·56	0·55	10,290	1715	588,900	2,355,600
" Y. Pine	34·87	0·56	8,454	1409	621,050	2,484,200

From the whole series of experiments, of which the briefest possible statement of the results obtained is here given, the conclusion may be drawn that:

The elasticity of yellow pine timber as used in construction is very variable, the modulus varying from one to three millions, the average being about two millions in small sections, and a little above one and a half millions in large timber (Tables I, IV, V).

The highest values are as often given by green as by seasoned tim-

* Laslett on Timber and Timber Trees. London, 1875.

† Calculated from Laslett's Values.

ber, and that, under sixteen square inches section and fifty-four inches length, at least, the magnitude of the modulus of elasticity is independent of the size of the piece (Table I).

The density of the wood does not determine the modulus; since the figure varies sometimes directly and sometimes inversely with the density, even where the wood is as nearly as possible in the same condition as to seasoning.

A high modulus usually accompanies high tenacity and great transverse strength, but it is not invariably the fact that maximum ultimate strength is accompanied by initial stiffness (Table I).

The pseudo moduli, determined by taking considerable deflections, are usually not greatly different from those determined from small deflections and light loads. The values of these moduli often decrease with increase in deflection (Table III).

An inspection of the woods tested plainly indicates, in the opinion of the writer, that the density of the pines is so considerably modified by the amount of pitch contained in the sap channels that it cannot be regarded as indicative of the strength of the timber. Where quite free from sap the wood usually exhibits increase of strength and elastic resistance to deflection, with increase of density.

The strength of timber, otherwise similar, is greatly affected by its structure, and the resistance offered to stresses applied transversely is greatest when the sections of the timber taken transversely exhibit most nearly vertical lines of grain.

The modulus of rupture by transverse stress varies, for yellow pine, from $R = \frac{3}{8}\frac{W l}{b d^2} = 10{,}000$ to $17{,}000$, the highest values being usually obtained from well-seasoned wood. An average value may be taken as $R = 13{,}000$ for good timber, which in the formula $W = C\frac{b d^2}{l}$ gives $C = 866$ pounds, or, practically, $W = 9000\frac{b d^2}{l}$, for good yellow pine.

The modulus of rupture varies as irregularly, and with as little regard to size or density of the material as does the coefficient for elasticity.

In the use of such materials, the only safe course for the designing and constructing engineer is evidently to adopt a moderate value of

the modulus in proportioning his work, and by careful inspection and test to secure the rejection of all material which is not of good quality.

As has been seen, careful inspection may sometimes lead to the selection of material—like the Navy Yard stock here described—twenty-five per cent. superior to the average of good timber, and fifty per cent. more valuable than the lower grades such as are often sold in our markets.

Reciprocal Displacements of the Halogen Elements.—It is well known that chlorine displaces iodine and bromine, and that bromine displaces iodine in metallic combinations; all these displacements are conformable to the thermic theory. The inverse displacements could not be executed under the same conditions, but they may be accomplished under special conditions, either by means of the dissociation of primitive compounds or even, when these compounds are stable, through the formation of secondary compounds, such as the chloride and bromide of iodine, the triiodide or the tribromide of potassium, or even the two chlorides of iron, of mercury, of copper, etc. When the compounds formed by the halogens are dissociated complete transformations are conceivable, following two opposite directions. Berthelot has shown that such transformations are possible in consequence of the dissociation of the secondary compounds, through a very interesting thermal mechanism, which he discovered by studying the inverse displacements of oxygen and of chlorine when united to arsenic, and of oxygen and iodine when united to potassium. It is necessary that the formation of the secondary compounds should set free a quantity of heat superior to the heat which is absorbed by direct substitution in the compounds of the first order. The substitution is also possible if the temperature be raised to the point when the metallic chlorides, bromides and iodides are dissociated, because the halogen element may then act upon a portion of the free metal; the opposite element, being partially removed in the meanwhile, is not present at the moment of cooling to reproduce its primitive combination. This second substitution, however, has not yet been accomplished without the intervention of the accessory influence of air, moisture and the constituents of glass.—*Comptes Rendus.* C.

THE ABSOLUTE ECONOMY OF ELECTRIC LIGHTING.

By Robert Briggs, C.E.

The following article is extracted from the pages of *Engineering*, Oct. 18, 1878, where it appeared in consequence of its being a part of a discussion before the British Association where the experiments on the electric light made by the Franklin Institute in 1878 were referred to. It has been republished at this late day in our pages so that the Journal shall include in its records the fullest statement of the condition of the art of electric lighting at the time when the experiments were instituted.

It is probable, at this time, the best adjustment of quantities of light to dimensions of coils and of wire and to the power expended, will give maximums of light produced per horse-power much in excess of those established by the committee. But the direction of inquiry and the course of argument in considering the results can never differ from the investigations and tenor of the report which the committee and the accompanying paper have established.

"The remarks made by me in the discussion of Mr. Shoolbred's paper on electric lighting, which he presented at the recent meeting of the British Association for the Advancement of Science, can be properly supplemented or extended by an inquiry into the relation of the expenditure of heat in the production of light by means of the burning of coal gas on the one hand or by electrical force on the other. These relations can be looked upon from several points of view. The first of these would be the purely theoretical one, and is based on the absolute heat which would be evolved under the condition of perfect combustion and absolute absorption of heat of coal gas in producing a given light, as compared with the heat which represents the same light as derived from the power expended in producing it.

"There is produced the light effect of one standard candle by the combustion of one-third of a cubic foot of coal gas each hour. That is, this result is that of the tests for quality of coal gas as established by law, but in practice of gaslighting with the usual imperfect burners not much above (if so much as) one-half the quantity of light is obtained. Accepting this quantity it can be asserted that the heat

effect of a cubic foot of coal gas is 690 heat units (pounds of water heated one degree Fahrenheit), and that a candle power of light is represented by 230 units.

"The Franklin Institute experiments did not give the light power claimed by most makers of dynamo-electric machines; they gave as the result only 380 candles of light, as proceeding from one horse-power, while the general claim for at least two of the dynamo-electric machines is nearly five times this value (200 becs Carcel being claimed for the Lontin machine and lamps as given by one horse-power. As each Carcel burner equals 9·6 candles of the English standard, it follows that this claim is for 1920 candles per horse-power). The Franklin Institute experiments also gave 31 to 38 per centum of the whole power as all that appeared in the resistance of the electric arc, which evolves the light, the remainder being expended in friction of machine and in the circuit, being dissipated as heat to the air probably.

"Taking the value of 380 candles as proceeding from one horse-power, we have, the force corresponding to one horse-power equals 1,980,000 pounds raised one foot high per hour—a unit of heat = 772 foot pounds ÷ one horse-power = 2565 units of heat per hour, and 2565 ÷ 380 gives 6¾ units of heat per candle.

"From which it appears that the theoretic expenditure of heat (or force) in producing gaslight is to that in producing electric light as 230 to 6¾, or as 34 to 1, while the theoretic expenditure of heat in producing gaslight is to that of the electric light in the arc alone, is very nearly as 100 to 1. All these are based on the Franklin Institute values; if the figures of M. Lontin are accepted the ratios become five times more favorable for electric lighting.

"But the heat which can be evolved by the burning of coal gas does not represent the heat which appertains to the coal that would have produced the same coal gas. Each pound of gas coal which has undergone the process of destructive distillation will have produced very nearly, on the average, 4 cubic feet of illuminating gas, and there will remain, as a residue of the process of gas making, about 0·85 pound of gas coke, and some 0·04 or 0·05 pound of gas tar, both of which substances are combustible. The quantity of coke, however, is reduced by the necessary use of coke in obtaining the heat for distillation until only 0·55 to 0·60 pound of unconsumed coke remains for each pound of coal originally charged in the retorts. Again, much of this coke is in the condition of dust or very fine breeze, so that practi-

cally only about 0·35 to 0·40 of gas coke is derived in merchantable or useful shape for each pound of coal originally treated; while the coal tar, although it possesses the constituents of a fuel, is not easily burned as such. Ordinary gas coal can be taken to have the value of 15,000 units of heat to each pound, and if it is supposed that from this quantity of heat there be deducted the heat value of, say, 0·6 pound of coke, taken at 13,000 units to the pound = 7800 units, together with the heat value of 0·05 pound of gas tar, taken at 20,000 units to the pound = 1000 units (total deduction 8800), we have 6200 units as the value of the fuel which yielded 4 cubic feet of gas. And, as before, taking each cubic foot of gas as evolving three candles of light, it follows that 517 units of heat will have been expended for each candle of light emitted, and this value should be taken as the expenditure of heat in lieu of the 230 units which the gas would produce after it is manufactured.

"This statement compared with the theoretic heat of electric lighting, as before estimated at $6\frac{3}{4}$ units per candle power, gives the heat in producing gaslight to that of producing electric light as 517 to $6\frac{3}{4}$, or as 76·6 to 1, while the theoretic expenditure of heat in producing gaslight is to that of the electric light in the arc itself as 230 to 1 — with the multiplication of these ratios by five to follow, if M. Lontin's development of light per horse-power is taken in place of that of the Franklin Institute.

"Unfortunately for this statement, in a practical point of view, the production of the electric light by the dynamo-electric machines involves the employment of motive power in the form of the steam engine (I omit to consider the gas engine as the source of motive power, thinking that its adaptation to general use should precede a discussion of its economy in this special application). And in none of the utilizations of natural forces is quite so great waste of original force as in the combustion of fuel in the steam engine. The best of engines can scarcely be claimed to give out a horse-power with less than 2 pounds of coal per hour, while the average of good engines of such size as would be at all available for power to run dynamo-electric machines of ordinary size, cannot be taken to average less than 6 pounds per per horse-power per hour. The former of these weights of coal represents 30,000 units of heat per hour to produce 2563 units of effect, or only 8·55 per centum of the expended fuel, and the latter only 2·85 per centum (one thirty-fifth) of the same.

"Taking this latter value as the practical one, the $6\frac{3}{4}$ units of heat per candle, which were the theoretic result of the Franklin Institute experiments, becomes 235 units of heat, in fact, needful to produce each candle. Our previous figures have given 230 units of heat as what would proceed from the best consumption of coal gas capable of producing one candle's light, and consequently the comparison of the heat practically demanded to produce electric light by the dynamo-electric machine actuated by the ordinary steam engine with the theoretic heat from coal gas is about 2 per centum against the electric system.

"But the previous figures also give 517 units of heat as what is expended in the making of coal gas for each candle's light, and this value compared with that for the dynamo-electric light with its motive (steam) power is 2·2 times less favorable for the system of gas lighting.

There is another way to look at the practical question. One pound of coal will produce 4 cubic feet of illuminating gas, each foot of which will have three candles' power; then omitting the other products of coal, 0·083 pound of coal represents the fuel expended in giving the light of one candle in the same time by coal gaslighting. Following the previous assumption that 6 pounds of coal will under the boiler of a steam engine evolve one horse-power, and taking the Franklin Institute value of 380 candles per horse-power from the dynamo-electric machine, then 0·0158 pound of coal represents the fuel in this case. This gives the practical relation of expenditure of coal (as fuel) in producing gaslight to that of the electric light under discussion = 5·25 to 1. This last view of the case is practically not an unfair one, for while the coke and the coal tar are not absolutely waste products in process of gas making, they are by no means profitable ones in themselves, and it is questionable if they pay much more than the cost of handling and disposing of them, especially at places where coal for fuel is relatively cheap.

"It must be noticed all through this paper that the assumption has been for the combustion of fuel by an ordinary steam engine and boiler at 6 pounds of coal per horse power, while a moderately good engine with boiler ought and will give a horse-power for 3 pounds of fuel, and the best engines will only require under 2 pounds for the same result, so that all the practical ratios are two to three times less favorable towards the electric system than they might be.

"It remains, therefore, that the most unfavorable statement of the

relative expenditures of heat and fuel practically, of the electric system by the dynamo-electric machines and steam engine, as compared to that of coal gas for equal quantities of light is, at present, 1 to 2·2; while in the processes there will have been burned as fuel one pound of coal under a steam boiler, against 5¼ pounds of coal treated in the retort for the manufacture of coal gas. Whether the cost of manipulation of the fires or the retorts, the wear and tear of machinery and engines employed in either processes, the interest upon the prime cost of apparatus, from the fire-places to the points for illumination may be in the same ratio as either the heat demanded or the coal used, is a question for further consideration and for future development.

"Coal gaslighting would seem to have attained nearly to its ultimate development and improvement. Minor advantages of cost of manufacture, and possibly in the direction of quantity of light evolved by a given consumption, can be confidently looked for. The public have been and are habituated to all the defects of the system, and from the beginning to the end they are grave and essential. On the other hand the electric light has the promise of the greatest improvement in methods of production, in its adaptation to defined wants, and in its relative cost, so that the unfavorable view of it which I have here presented should not be taken, and it is far within the limits of probability that the best statement I have presented underrates its possibilities.

"It may be proper to advert here to the low result in candle-power for the electric light found by the Franklin Institute experiments. The power of light emitted by the candle or the gas-burner usually tested by it, is so small that the radiant light from the sides of a dark chamber may be neglected. The measurement of light was taken in all instances from the rays of a standard candle as emitted on the horizontal plane through the centre of the flame. It was found first, that this radiant light upon the walls of a dark box from the electric light was very great, and the means shown in the report of the committee (where it will be seen that only a pencil of light of not over a single degree of aperture was directed into the box or tube carrying the photometer) were found effectual to give a highly satisfactory comparison of the greater with the lesser source of light under investigation. Next it was found in the carbon experiments that there was an apparent unsteadiness of light of great amount emitted from the carbon. As the experiments proceeded it became evident that this unsteadiness was

occasioned by the formation of an arc upon one side of the point (in place of a symmetrical brush), which traversed around the points as they wore away, emitting much the largest part of the light through a small arc of radiation in the horizontal plane, and presenting on one side of the points a brilliant reflection from them, and a corresponding obscuration on the opposite side. This difficulty was met and overcome by the adaptation of carbons of suitable sectional dimension for the current of electricity employed, until a uniform brush, emitting rays of very nearly equal value in all directions, was obtained. The Franklin Institute experiments consequently do not give the value of an apparent maximum of effect bnt the mean effect from the electric lamps experimented with.

"For a practical datum it may be well to state, in conclusion, that the result of 380 candles per horse-power is, that 90 pounds weight falling one foot per minute represents the power demanded to afford a light of the best spermaceti candle by means of the dynamo-electric system. A man ascending a flight of steps at very moderate rate will have exerted a force equal to three candles. And the results claimed by many makers of dynamo-electric machines are four to five times as good as these.

"The problem of electric lighting to-day is the production of small enough quantity of light with equal economy to that attained for large illuminating effects, and of the distribution into small sources of light of the great light capacity now obtained at a single point of emission. To these ends the intelligence of all electricians is now directed, and the attainment of some measure of success can be confidently anticipated."

Coret's Thermometer.—Coret solders end to end many concentric tubes of different metals, steel and zinc for example. By alternating the joints the differences of dilatation are added, so that the last tube, being connected with a toothed wheel or series of levers, gives a great motion to a needle, sufficient to indicate small fractions of a degree of temperature. The metals being good conductors, the indications are rapid when the metallic mass is placed in contact with any body of which the temperature is desired. The tubes can be concentrated in a space of less than two centimetres (·79 inch). The thermometer is, therefore, very convenient for medical use.—*Bull. de la Soc. d'Encour.*

C.

A NOTE ON THE ARTIFICIAL PRODUCTION OF DIAMONDS BY THE PROCESSES OF DESPRETZ.

By Prof. Edwin J. Houston.

In 1849 Despretz endeavored to produce diamonds artificially by the direct fusion of carbon. For this purpose he combined the heat of the voltaic arc, the sun's heat, concentrated by means of a large lens, and the flame of a powerful blowpipe. The pile consisted of 185 pairs of Bunsen cells. Fragments of nearly pure anthracite, placed in this powerful heat focus, he claims, were observed to melt and fall in drops. He gives an account of these experiments in a note read before the French Academy of Sciences at the séance of June 18, 1849.*

Later, in 1849, he obtained undoubted evidences of the volatilization of carbon by exposing it to the heat of a voltaic arc obtained from a battery of 496 elements, arranged in four parallel series.†

In 1850, by means of a battery of 600 Bunsen elements, connected with a second battery of 135 elements, he succeeded in partially fusing a piece of hard retort carbon, which was thereby converted into pure graphite.‡

By none of these experiments, however, was he able to obtain any positive evidence of the crystallization of carbon.

Notwithstanding these failures he continued his experiments for several years. Abandoning the methods of sudden fusion and volatilization, he tried a number of experiments, all based on the gradual deposition of volatilized material. In one of the most successful of these experiments, the carbon was slowly volatilized in a vacuum, by the continuous discharge of a Ruhmkorff induction coil. A small cylinder of pure carbon placed at one electrode, was gradually deposited in a crystalline state on platinum wires attached to the other electrode. The electrical discharge was continued without interruption for one month, at the end of which time, by the aid of a microscope, octahedral crystals were plainly visible.

* *Comptes Rendus*, Tome 28, p. 755.
† *Comptes Rendus*, Tome 29, p. 48.
‡ *Comptes Rendus*, Tome 30, p. 367.

Apparently no commercial application was ever made of these processes, no doubt on account of the length of time required for the deposition. The present advanced state of electrical science, however, may before long suggest some modifications of these processes that may make them more practicable.

The public interest in the artificial production of the diamond will, I think, warrant my translating the first part of M. Despretz's original paper containing a brief account of the process. It is entitled:

"OBSERVATIONS ON CARBON, AND ON THE DIFFERENCE OF TEMPERATURE OF THE LUMINOUS POLES PRODUCED BY INDUCTION.*

"The results of the experiments that I had the honor of presenting to the Academy, some few years ago, on the fusion and volatilization of certain bodies, especially carbon, show that it is neither by fusion nor by the sudden volatilization of carbon, that we may hope to obtain this body crystallized.

"We have shown by these experiments that pure carbon melted, and that the diamond melted, form only amorphous graphite; that carbon suddenly volatilized on the walls of a flask gives only a dark powder without any appearance of crystallization.

"These experiments on the fusion and volatilization of carbon are now repeated every year in the course on chemistry and physics at the Sorbonne. We can obtain by the heat of the arc a number of crystallized bodies. I hope I may return to this point. The same results would probably be obtained for carbon if we had crucibles less fusible than this substance, which we do not possess. I have, therefore, had recourse to another process. The process which has given me the best success is founded on a gradual volatilization produced by induction currents.

"I took a flask with two tubulures, arranged as for the electrical egg; to the lower rod I attached a cylinder of pure carbon, a few centimetres in length and one centimetre in diameter. I attached to the upper rod a dozen thin platinum wires; I produced a vacuum in the flask; then, the distance between the wires and the carbon being from 5 to 6 centimetres, I caused to pass the current of an induction apparatus constructed by M. Ruhmkorff, and which has been described in several notes presented to the Academy. The arc was reddish from

* *Comptes Rendus*, Tome 37, p. 369.

the carbon to within a short distance of the platinum; the portion which enveloped the extremity of the platinum wires was of a bluish-violet.

"This arrangement of apparatus was always adhered to. We placed the bundle of platinum wires above, in order not to confound the minute sparks from the carbon, with the crystals which we wished to form.

"The pile was composed of four Daniell's elements, connected two by two.

"The experiment continued uninterruptedly for a month, excepting the time necessary to recharge the pile. There was deposited a thin black layer of carbon on the wires. This layer, viewed by a lens, did not present any very distinct appearance; with a compound microscope, with a magnification of about thirty times, it presents several interesting points.

"I saw over the wires, and particularly at the extremities, particles separated one from another, which seemed to belong to the octahedral system.

"I saw, also, on the black layer, but none at the extremities, several small octahedra resting on a summit.

"I repeatedly examined the wires, and always found the same things.

"An able and skilled crystallographer has recognized black octahedra, truncated at the extremities, and little white octahedra resting on a summit.

"I did not previously inform my colleague, M. Delafosse, what I observed.

"I substituted for the wires a polished platinum plate, 1½ centimetres in diameter; although this experiment continued in activity for almost six weeks, yet there was no deposit of crystals on the plate. The plate was covered over at least half of its surface with almost circular curves, with radii somewhat larger than that of the plate; each of these curves was painted with one of the colors of thin films. We saw here and there small greyish-white spots of powder, which appeared to be the result of the momentary adherence of isolated deposits.

"In another experiment I fixed a cylinder of pure carbon to the positive pole of a feeble Daniell pile, and to the other pole a platinum wire; I then plunged the two poles in water slightly acidulated. The

experiment continued for more than two months; the negative pole was covered with a black layer.

"Nothing was discovered in this layer by an examination with a microscope.

"I requested M. Gaudin, known to the Academy by different researches, to try both products on hard stones.

"He proved, in my presence, that the small quantity of material that surrounded one of the dozen wires of platinum, mixed with a little oil, sufficed to polish in a short time several rubies.

"The black powder deposited in the humid way, although much more considerable in quantity, required more time to produce the same polish.

"We know that the diamond is the only body which polishes the ruby; therefore M. Gaudin has not hesitated to consider both materials as the powder of the diamond.

"I have made a great many experiments during two years; I give here the two which afford the most interesting results."

In connection with the above we note a communication to the French Academy of Sciences,* from M. Lionnet, in which he claims to have made, in 1846 and 1847, experiments on the crystallization of carbon. These experiments, since forgotten, were recalled by a communication of Chancourtois † to the Academy. M. Lionnet claims to have used the following process: A voltaic couple, formed of platinum and tin, was plunged in a small vessel filled with carbon bisulphide, under the action of the weak current so developed, decomposition of the electrolyte occurred, the sulphur combining with the tin, while the carbon, the author claims, was deposited in a crystalline condition on the bottom of the vessel. It would seem that if deposited at all under these conditions, it would be on the surface of the platinum.

Central High School, Philadelphia, Aug., 1880.

Lunar Geology.—J. Landerer has submitted to the Academy a work in which he seeks to determine the lithologic character of our satellite. He thinks that the density of the moon, and the angle under which it polarizes the light of the sun, are such as to show that the materials of the surface are analogous to those of the silicate rocks.—*Comptes Rendus.* C.

* *Comptes Rendus*, Tome 63, p. 213. † *Comptes Rendus*, Tome 63, p. 22.

THE PERKINS ENGINE.

The system of steam engine and boiler construction so perseveringly developed by Mr. Loftus Perkins has during the past few years attracted considerable attention, and a strong desire has been expressed in many quarters to have made public some really definite data as to the capabilities of the system. Under these circumstances the report by Mr. F. J. Bramwell, which is hereto attached, will be read with much interest. This report deals with the results of a trial made by Mr. Bramwell of the Perkins engines fitted to the yacht *Anthracite*—a vessel whose recent transatlantic voyage has been noticed in this journal. The trial lasted twelve hours, and appears to have been conducted throughout with great care and under rigorous conditions, and the results so far as recorded are no doubt perfectly trustworthy. It will be seen by the report that during the period of 11 hours and 10 minutes the engines developed on an average 80·55 horse power, while the average coal consumption during the whole trial (including getting up steam) was 1·83 lb. per indicated horse power per hour, the consumption during 8 hours 55 minutes, during which the engines were developing 80·9 horse power, being estimated by Mr. Bramwell as 1·7 lb. per indicated horse power per hour. These are undeniably excellent results, but they are scarcely so good as engineers have been led to expect from the Perkins engine working with a boiler pressure of 340 lbs. to 370 lbs. per square inch.

It is somewhat unfortunate that Mr. Bramwell in testing the Perkins engine should have adopted the coal consumption as the sole measure of economy, as by his doing so it is rendered impossible to separate the performance of the engine and boiler. It is possible that Mr. Perkins may have preferred his system of engine and boiler construction tested as a whole; but however this may be, it is to be regretted that during the trials of the *Anthracite* observations were not made as to enable an opinion to be formed of the practical value of steam at very high pressures, apart from any question of boiler efficiency. Under the circumstances, all that can be done is to attempt to form some idea of what the efficiency of the boiler really was. In Mr. Bramwell's report the dimensions of the boiler of the *Anthracite* are not recorded, but through the courtesy of the Perkins Engine

Company we are able to give them. The boiler, then, is 14 "sections" in width, while there are 7 rings of tubes in the firebox and 10 layers of tubes above, the straight tubes above the firebox being 4 ft. 7 in. long. The tubes are 3 in. in diameter outside and 2¼ in. inside. A reference to the engravings of the Perkins boilers of the yacht *Wanderer*, which we published on page 472 of our 28th volume, will make the general arrangement clear. The boiler of the *Anthracite* has altogether 633 square feet of heating surface and 15 square feet of fire-grate, the ratio of grate to heating surface being thus 1 : 42·2. The total coal consumption recorded by Mr. Bramwell is 1697 lbs. during the trial of 12 hours 3 minutes, it being thus at the rate of 9·4 lbs. per square foot of grate surface per hour, or 0·222 lb. of coal per hour per square foot of heating surface. The boiler was thus very easily worked, and considering the high quality of coal used, and the fact that the feed was drawn from the hot well, there is every reason to believe that the evaporation per pound of coal was high. Probably we shall be erring on the side of moderation if we put down the evaporation at 10½ lbs. of water per pound of coal.

The mode of trial adopted by Mr. Bramwell does not admit of the coal used in getting up steam being separated from that actually expended in making steam during the run, but for our purpose we may assume the coal consumption estimated by Mr. Bramwell as taking place between 7.50 A.M. and 4.45 P.M., viz., 1·7 lb. per indicated horse power per hour. With an evaporation of 10½ lbs. of water per pound of coal this would give a consumption of $1{\cdot}7 \times 10{\cdot}5 = 17{\cdot}85$ lbs. of steam per indicated horse power per hour. This is a very low consumption, particularly when the moderate size of the *Anthracite's* engines is considered, but we cannot help thinking that it would be very closely approached, if, indeed, not equaled, by a compound engine of the modern type working with steam at 80 or 90 lbs. pressure. In saying this it is only fair to add that we consider the *Anthracite's* engines were not tested under circumstances calculated to show the highest economy as far as the consumption of steam per horse power is concerned. We believe that they would have done better if they had been more heavily loaded; as it was, the power developed (notwithstanding the high boiler pressure) only about equal to that which would have been developed by an ordinary compound marine engine with the same size of low-pressure cylinder, working at the same piston speed, and with a boiler pressure of 70 or 80 lbs. per

square inch. An inspection of the indicator diagrams taken from the engines of the *Anthracite* by Mr. Bramwell, shows that excessive throttling took place between the boiler and the high-pressure cylinder, the difference between the boiler pressure and the initial pressure in the cylinder being rarely less than 170 lbs. per square inch. Thus, with a boiler pressure of 340 to 370 lbs. per square inch, the initial pressure in the high-pressure cylinder was always under 200 lbs. Of course the low power at which the engines of the *Anthracite* were worked during Mr. Bramwell's experiments was favorable to the performance of the boiler, and it is quite possible that, taking the engine and boiler together, the load may have been well chosen to secure a low fuel consumption. At the same time, the work demanded from the engines and boiler during the trial was undoubtedly much easier than that which steamship machinery is generally called upon to perform in regular practice.—*Engineering.*

---

37 GREAT GEORGE STREET, WESTMINSTER, S. W.
June 28, 1880.

To the Directors of the Perkins Engine Company, Limited.

GENTLEMEN:—In accordance with your desire, as communicated through Mr. Loftus Perkins, I, on Saturday the 22d May, made a trial of the Perkins patent engine fitted on board the screw steam yacht *Anthracite*, preparatory to her leaving England on her voyage to New York.

You are of course perfectly well aware of the nature of the engines and boilers of the *Anthracite*, but in order that this report may be more readily followed, I think it well to state, in the report itself, the leading particulars of the machinery of that vessel.

The engines are of the direct-acting inverted type with surface condensation. Two cylinders only are used, but the after cylinder has two diameters of bore; the upper and smaller one is the high pressure, and receives the steam from the boiler during the first half of the down stroke, the lower and larger diameter, is the medium or intermediate, and is supplied at the up stroke with the steam, which in the high pressure did the work of the preceding down stroke. The exhaust from the bottom of the after cylinder passes into a chamber, from which is afforded the supply to the low pressure (the forward) cylinder, and from this arrangement there is obtained in two cylinders an expansion of thirty-two times.

The distribution of the steam in the after cylinder is made by three lifting double-beat valves, the upper faces of these valves being, however, divided into two sections. The valve stalks rise and fall by the action of eccentrics, which control the motion of the stalks downward as well as upwards, the stalks being loose in close-topped sockets in the valves, so that after the valves are seated the stalks can recede from them. The distribution of the steam to the low pressure cylinder is made by an ordinary slide valve with an expansion valve on its back; this latter valve is worked off the prolongation upwards of the circulating pump rod. The surface condenser is composed of a number of close-topped, galvanized, wrought-iron tubes, standing vertically from a tube plate, and having within them smaller tubes open at both ends, and proceeding upward from a lower tube plate, so that the water from the sea passes up through the central tubes, and down the annular spaces to the inlet of the circulating pump.

The exhaust steam is admitted into the body of the condenser, and comes into contact with the exterior of the close-topped tubes, the condensed steam being drawn off by the air-pump, and returned to the hot well which surrounds the upper part of the condenser. The space between the high pressure piston and the upper side of the intermediate piston is in connection with the chamber which supplies the low pressure cylinder. The circulating pump is worked by a beam off the low pressure piston rod, and the air-pump is worked by a similar beam off the intermediate cylinder piston rod. The feed pumps and bilge pumps are worked off the cross heads of the circulating pump and of the air-pump. The reversal is effected by a link motion.

The cylinders and their covers are heated by steam circulating through wrought-iron pipes cast into the thickness of the metal, and are very efficiently clothed so as to prevent loss of heat.

The boiler is the well known Perkins boiler formed of successive horizontal rows of wrought tubes (3 inches external diameter) connected at frequent intervals by vertical thimbles, the whole series being contained in a wrought-iron double casing having the space filled in with vegetable black. The water gauges, one in the boiler and one in the engine room, are made of flat plates of mica secured in frames. The boiler is supplied with distilled fresh water. There is a still fitted with a coil, and having its steam pipe in connection with the condenser, by which the store of water can be distilled over for use.

With respect to the dimensions of the engine, I was furnished at

my request by Messrs. Hawks, Crawshay & Sons, the builders of the engines, with the working drawings. These drawings show the high pressure cylinder to be 8 inches bore, the intermediate 16 inches bore, and the low pressure 23 inches bore, but I was informed by Mr. Perkins that the cylinders were all somewhat smaller than the foregoing dimensions. He has now sent me the gauges, and these give the high pressure cylinder at $7\frac{3}{4}$ inches diameter, the intermediate as $15\frac{13}{16}$ inches, and the low pressure as $22\frac{13}{16}$ inches. The stroke in both cylinders is 1 foot 3 inches. The diameter of the piston rods (the areas of which have to be deducted from the area of the intermediate piston, and from that of the underside of the low pressure piston) is $2\frac{3}{4}$ inches.

My principal assistant was on board the *Anthracite* on the Friday, and he informed me that the fire was let out at about 2.30 P. M. on that day, and as the result the boiler and water were practically cold when the fire was lit on Saturday morning.

Preparations for the trial had been made by weighing out 50 cwt. of "Nixon's navigation hand-picked lumps" into fifty 1 cwt. sacks. These were ranged on deck. The bunkers, which were full, were sealed up both above and below. A spring balance was hung up on deck, close to the stokehold hatch, and one of my assistants caused each sack of coal to be reweighed just before it was lowered for use. The gross weight was taken, and the sacks were afterwards separately weighed so as to obtain the net weight of the coal.

Fifty pounds of dry wood were served out and two sacks of coal, and with these the fire was laid (the grate has an area of about 15 square feet). The fire was lit at 6.28 A. M. Steam was up and the engines were turned round at 7.18 A. M. The height of water in the boiler gauge was noted, and also the height in the hot well, the still cock being shut, was sealed in that position. The steam stop valve was sealed in its wide open position. The engines were started, and the vessel got under way at 7.20 A. M. The throttle valve was put into the position which the engineer knew, from experience, would cause the engines to run at about 150 revolutions per minute after they became thoroughly heated up, and the handle was sealed into this position, the link motion being in full gear ahead. The engines were provided with a Harding's counter, such as is used in the Navy, and there were pressure gauges to show the boiler pressure, and the pressure in the chamber supplying the low pressure cylinder, and the condenser was provided with a vacuum gauge. Four Richard's indicators were

fitted, viz.: one to the high pressure end of the after cylinder, one to the intermediate end of that cylinder, one to the top and one to the bottom of the forward, the low pressure cylinder.

The first set of diagrams was taken at 8.22 A. M. The first reading of the counter at 8.30 A. M. From this time until 5.45 P. M. the counter was read at the hours and half hours, and sets of diagrams were taken at the quarter past the hour, and at the quarter to the hour. The time when each sack of coal was lowered into the stokehold was noted, and also the time when the stoker commenced to use the contents of each sack.

The last shovelful of the fifteenth sack was put on at 5.18 P. M., and it was decided to stop the trial as soon as the coal then in the fire was exhausted.

I determined to let the engines run on until they stopped of themselves, and to take indicator diagrams, first at each quarter of an hour and then at each five minutes during the time they were gradually stopping. The quarter hour diagrams were taken until 6.30 P. M., when the engines were making 124 revolutions, and the five minutes diagrams were taken until the engines came to a stand at 7.23 P. M. or 12 hours 3 minutes after their start in the morning. The water in the boiler was pumped to the same level in the gauge as that at which it had stood in the morning and the height of water in the hot well was noted.

The mean revolutions from 8.30 A. M. to 6.30 P. M., 10 hours, were 130·77 per minute, and from the first start to the same time being 11 hours 10 minutes, the mean revolutions were 130·4 per minute.

An experiment was made as to the transmission of heat through the boiler casing. This was carried out by placing the bulb of a thermometer in contact with the top of the casing, the bulb being covered over with cotton waste; the result was that at the end of one hour the thermometer stood at 125 degrees Fahr.

From the start at 7.20 A. M. to 6.30 P. M., 11 hours and 10 minutes, the engines developed an average gross indicated horse-power of 80·55, but from 6.30 to the time, 7.23 P. M., that the engines stopped of themselves from the fire having burnt itself out, the power was of course gradually diminishing. This being so I have calculated the whole work from first to last in foot-pounds; these amount to 1,828,291,466 foot-pounds, equivalent to 923·38 horse-power if exerted for one hour.

I estimate the 50 pounds of wood as of about one-third the value of coal as fuel, say .	17 lbs.
The coal 15 cwt., . . .	1680 "
	1697 "

The gross indicated horse-power developed during the trial being equivalent to 923·38 horse-power exerted for an hour 1697 ÷ 923·38 = 1·83 pounds of coal per horse power per hour, *including the coal used in getting up steam.*

As a check upon the foregoing from the time when the first shovelful of the fourth sack of coals was put on the fire, namely, at 7.50 A. M. to the time when the first shovelful of the fifteenth sack of coals was put on the fire, namely, 4.45 P. M., *i. e.*, 8 hours 55 minutes, the engines exerted an average of 80·9 horse-power; this gives the consumption per horse-power per hour at 1·7 pounds, assuming the fire was uniform at the commencement and end of these times. The loss of water for the whole 12 hours was 23½ gallons.

The engines worked with the most remarkable smoothness and regularity, as with the exception of tightening up the bolts of two glands about an hour after the start, there was not a spanner or hammer or any tool used about the engines during the 12 hours the ship was under way, nor was there a single handle connected with the engines shifted. The link-motion was in full gear the whole while, and the stop valve and throttle valve remained, as I have said, sealed up and unmoved.

Water was allowed to circulate on the thrust-block, but there was not a heated bearing anywhere. The amount of lubrication was small, involving an expenditure of about one gallon of lard oil, while cylinder and slide lubrications are in the Perkins system inadmissable and with the metal used unnecessary.

The engines were thoroughly steady, no straining or racking of any kind, in fact it is impossible for any pair of engines to have worked in a more thoroughly satisfactory manner, so far as all external evidence of working was concerned, and there were no sounds to lead to any other belief than that of all being satisfactory within.

I hope on the return of the *Anthracite* from America to have an opportunity of seeing the engines opened, and of thus observing their condition after some thousands of miles of steaming, and I should also be glad to have an opportunity of retesting the consumption.

At the conclusion of the trial my assistants took away the four indicators and the spring balance with which the coals were weighed. All these have been tested with the result that the balance and the 100 pound spring (used in the high-pressure cylinder indicators) and some of the lighter springs, are absolutely accurate, and that the variations in the others are too trifling to call for any allowance in calculating the mean pressures.

It may be well to state that the mean pressures of the various diagrams (of which there were 128 in all) have been ascertained by dividing the areas of the diagrams, obtained with the planimeter, by the lengths of the diagrams. This is a thoroughly accurate mode, while the plan commonly followed of measuring the heights of the diagrams at a number (usually ten) of places is one which ordinarily gives a pressure in excess of the truth.

It would be an investigation of considerable interest in an engineering point of view if a vessel fitted with first-rate compound engines of the ordinary construction, and working, say, at 70 pounds pressure in the boiler and indicating something about the same power as those of the *Anthracite*, viz., from 70 to 90 horse-power, could be tried in a precisely similar manner to that in which the *Anthracite* was tried.

In conclusion I wish to say that I esteem myself particularly fortunate in having obtained the friendly co-operation during the run of Mr. William Rich, of Messrs. Eastons and Anderson, whose long experience in engine trials rendered his assistance of the greatest value, and to whom I beg leave to express my best thanks. I have his authority for saying that he entirely concurs in this letter of report.

I am, gentlemen, your obedient servant,

(Signed) F. J. Bramwell.

Ammonia in Vegetables.—H. Pellet has lately found appreciable quantities of ammonia in various kinds of grain, as well as in beet root. In wheat the weight of ammonia is one-tenth as great as that of the ashes. He thinks that magnesia and phosphoric acid enter into vegetables under the form of an ammoniacal magnesian phosphate, which is very soluble in all vegetable acids. He is now studying the effects of ammoniacal manures, and of nitrogen, under different forms, upon the amount of ammonia in vegetables.—*Comptes Rendus.* C.

THE MANUFACTURE OF BESSEMER STEEL AND INGOT IRON FROM PHOSPHORIC PIG.

By C. B. HOLLAND and A. COOPER, Sheffield.

Read before the Iron and Steel Institute, May, 1880.

Before entering upon the subject of our paper it might not be out of place to consider for a moment what is steel and what is ingot iron. Steel has been defined as "an alloy of iron and carbon which is capable of being cast whilst in a fluid state into a malleable ingot," and all other elements usually found in the steel of commerce, such as silicon, sulphur and phosphorus, may be regarded as impurities, and are more or less hurtful. In like manner, ingot iron may be defined as an iron which is capable of being cast whilst in a fluid state into a malleable ingot, and other elements found in it (including carbon) may in this case also be regarded as impurities. It follows, then, that those steels and ingot irons are the best and purest which contain the noxious elements in the least quantities, no matter whether they be produced from the finest brands of Swedish and hematite, or from common iron containing from 1¼ to 2 per cent. of phosphorus. And we think all will agree, if it can be shown that steel and ingot iron can be produced from the latter kind of pig of the same chemical composition, and capable of standing the same mechanical tests as that produced from the purer irons, that one is as good as the other for all purposes. It is not our intention to occupy the time of the Institute by referring to any of the numerous papers that have been written, and the theories that have been propounded, during the last eighteen months, on the dephosphorization of iron by the Thomas and Gilchrist process, interesting and instructive as many of them are. It will be sufficient to say that notwithstanding the great strides that had been made in the development of the process at the works of Messrs. Bolckow, Vaughan & Co., under the able direction of Mr. Richards (who must always be regarded as one of its earliest pioneers), and the very satisfactory results obtained by that company in the manufacture of steel from Cleveland pig-iron up to the early part of November last, before we had seen the process in operation, there appeared to us certain difficulties which we feared would greatly retard its successful

working from a commercial point of view. The first of these may be regarded as a technical one, and had reference to that part of the operation now well known as the "after-blow." Assuming, as we did, all the metalloids, with the exception of phosphorus and sulphur, to be oxidized before the commencement of this period, it seemed doubtful to us whether this would be practicable (having no definite point at which we could safely stop blowing corresponding to the drop of the carbon flame in the ordinary process) to burn out the whole of the phosphorus regularly, without sometimes carrying the operation too far, and thereby oxygenating the charge. And this, as all steelmakers will agree, is very apt to give trouble. Again, we had our misgivings about the gathering at the nose, concerning which we had heard so much, and the delays which we thought must necessarily result from the indispensable and repeated turning down of the converter for sampling. It was at this time (early in November last year) that we were invited to see the process at the Hœrde Works in Westphalia, and through the courtesy of Mr. Pink, the manager of the Steel Works, our representative was not only shown the process in operation, but every information respecting the difficulties that had been experienced up to that time, and the means which had been employed to overcome them, was very kindly given to him by that gentleman. From the working at Hœrde it was apparent that the phosphorus was eliminated with regularity—there were no violent reactions on the addition of the spiegel, showing that the metal was not oxygenated to any great extent, and the steel made was of a mild quality, and very malleable. On November 20th we commenced to work the process in Sheffield, using a mixture of White Lincolnshire and No. 4 forge irons. By sampling during the after-blow, we were enabled not only to remove the phosphorus in a very satisfactory manner, but also to make good and malleable steel; but the gathering of the slag and metal at the nose of the converter at first proved such an obstacle to rapid working, that although we employed men with long bars to fettle after each operation, at the end of from twelve to eighteen blows we were compelled to stop, allow the converter to cool down, and cut out the accumulation. At this early stage we were of opinion that by increasing the area of the nose we should get a decreased pressure of gases, and consequently the slag and metal would not be carried up so high, and that this would remedy the evil. Our experiment proved to us conclusively the fallacy of this, for after altering the shape of our con-

verter so that the area of the mouth was doubled, and lining up with basic brick as before, although we worked under precisely the same conditions, this converter was completely slagged up to the nose, and was unfit for further work after ten blows, simultaneously with the other converter and with the mouth of usual size, lined with basic bricks. We tried admitting blast at the throat, with the view of burning the carbonic oxide, and so increasing the temperature at this point, but the results were not sufficiently encouraging to justify our following up the experiment. In the succeeding week we lined our converter nose with brick made of silicate of soda and limestone, the other with ordinary fireclay bricks, contracting both at this point to a diameter of about 20 inches, or to about one-half the original area. In both cases we perceived at once a great improvement through the reduced size, the converters retaining their heat very much better than before. The slag still adhered to the first-mentioned slightly. It was easily removed by bars, but, unfortunately, usually carried a portion of the very brittle silicate of soda and limestone brick with it, and this form was abandoned on that account. The other converter with the fireclay bricks gave better results, and we have since used them regularly. What little slag adhered could be removed without any material injury to the lining. Although the common fireclay bricks then used wasted considerably, numerous experiments proved that the amount of silica carried down into the bath from them was too small to do any harm, and as the lining at this part was about 15 inches in thickness, we were able to get regularly from 30 to 40 blows before it got too thin. Thus, as far as the immediate nose was concerned, the difficulty seemed at an end, but we soon found that a great accumulation took place just below the junction of the fireclay bricks with those of the basic material, and also along the sides of the converter, in the form of a ridge of slag and metal left little by little on each turning down for the purpose of sampling. It seemed to us that to remedy this evil the simplest plan was to avoid testing as much as possible, if not altogether. All our results had shown that notwithstanding the after-blow the fully blown metal was not nearly so much oxygenated as at the end of the ordinary blow in the hematite process. Numerous analyses gave us the impression that this was due to the presence of manganese in the mixture we were then using, containing as it did about ·75 per cent. of this metal, and we found invariably

that when we started with this quantity there always remained from ·1 to ·2 at the expiration of the after-blow.

The following analyses show how much more readily (during this part of the operation) the phosphorus is attacked than the manganese.

	At the drop of the carbon flame.	At the end of the after-blow.
Blow 108 Phosphorus,	·883 per cent.	·062 per cent.
" Manganese,	·443 "	·111 "
Blow 136 Phosphorus,	1·090 "	·044 "
" Manganese,	·183 "	·147 "
Blow 166 Phosphorus,	·890 "	·081 "
" Manganese,	·435 "	·194 "

These, amongst other results of a similar kind, led us for a time to think that it might be possible to eliminate the whole of the phosphorus by increasing the amount of manganese in the charge, and blowing with the spectroscope until the absorption bands—which one of our greatest authorities, Mr. Watts (see Roscoe's *Chemistry*, vol. ii, part 2, page 77), on the use of this instrument has attributed to the oxides of this metal—had disappeared, for although our previous experience with mixtures containing ·75 manganese had shown us that the bands always disappeared from the spectrum at the drop of the carbon flame, at which from ·2 to ·4 still remained, we thought it likely (assuming the above theory to be correct) that with a larger quantity we should be able to get rid of the phosphorus before these bands vanished. Accordingly, by means of ferro-manganese we increased the proportion of manganese in charge No. 165 to about 1·75 per cent. At the drop of the carbon flame, when the bands disappeared, we found the composition to be:

Sulphur,	·118 per cent.
Phosphorus,	·955 "
Manganese,	·817 "

The sample taken at this period hammered well, but, on testing in the usual way, proved very brittle indeed, and the fracture did not show the characteristic appearance indicating the presence of phosphorus, but resembled that of a very hard steel. On turning up the converter again for a further blow of thirty seconds certain bands reappeared in the green portion of the spectrum for three or four seconds only; the composition at the end of this period was found to be:

Sulphur,	·155 per cent.
Phosphorus,	·831 "
Manganese,	·612 "

The sample taken and treated in the same way as before was still exceedingly brittle, and resembled the former one. Again the converter was turned up, and for the space of one or two seconds only these bands again flashed across the spectrum, after which they were not observed during the remainder of the after-blow, although most carefully looked for. At the end of this period the test made was still hard, though much milder than before, and had the following composition:

Sulphur,	·132 per cent.
Phosphorus,	·460 "
Manganese,	·576 "

The charge was then blown for a further sixty seconds, and as the sample, which was very malleable, did not harden, thinking that the fine crystalline appearance of the fracture on breaking was due to manganese, the heat was cast without the addition of spiegel or ferro-manganese. It showed no disposition to rise in the open-top ingot moulds in which it was cast, and a tire 2 feet 8 inches inside diameter, made from one of the ingots, all of which hammered very well indeed, deflected 8 inches before breaking. Analysis of this steel or ingot iron, which showed that we had not carried the process quite far enough, gave the following composition:

Sulphur,	·114 per cent.
Phosphorus,	·146 "
Manganese,	·440 "

From these results we drew the following conclusions: That the absorption bands usually seen in the spectrum of the Bessemer flame are due to carbon, and that manganese bands (very similar in appearance to those of carbon) are apparent only when that metal exists in quantities of about 5 per cent.; and further, that we could not hope, through the agency of this metal to determine upon any fixed point indicated by the spectroscope at which we could safely stop blowing, satisfied that the whole of the phosphorus was removed. But we had before noticed that with regular charges and conditions the after-blow was of very constant duration, and the results of the blow showed us not only that with 1·75 per cent. of manganese in the charge, in order to be quite sure of eliminating the whole of the phosphorus, we might safely blow rather longer (without risk of oxygenating) than the time actually required to effect this purpose, and thus avoid sampling; and afterwards, for rails, add a low spiegel or plate iron to carbonize; but

also that, if an ingot iron were required, it could be readily produced by the means described, without any such addition. Want of an iron containing anything like the amount of manganese prevented us from following up these results for some time, but meanwhile, with mixtures such as we had, containing ·75 per cent., we tried the effect of blowing after the addition of the spiegel, until the carbon bands, as seen through the spectroscope, had nearly vanished. The chemical analyses and results of a rolled bar from each of three such blows, tested, gave the following results:

Blows.	C. Carbon by combustion.	Sulphur.	Phosphorus.	Manganese.	Breaking Strain. Tons.	Elongation in 10 in. Per cent.	Reduction of area. Per cent.
·314	·040	·065	·030	·356	24·20	26·875	51·78
·329	·050	·045	·051	·240	26·48	29·375	65·59
·330	·038	·069	·035	·266	24·18	26·250	59·04

Three more blows made, amongst others, in the same manner, some weeks afterwards, gave the following results:

Blows.	C. Carbon by combustion.	Sulphur.	Phosphorus.	Manganese.	Breaking strain. Tons.	Elongation in 10 in. Per cent.	Reduction of area. Per cent.
·689	·061	·096	·051	·345	Not tested.		
·703	·073	·091	·027	·370	24·49	31·25	49·79
					24·62	27·50	56·06
·705	·101	·070	·047	·468	Not tested.		

The metal in all cases gave no trouble in casting into ingots (some, indeed, were cast in open-top moulds from the top), and it was very malleable. Specimens of bars from blows 314, 329, 330 and 703 plunged into cold water at a red heat—some of which were afterwards bent and some twisted, cold—are exhibited. Having at length obtained some iron containing about 2 per cent. of manganese, a charge of this was blown alone, and at the end of the after-blow of 130 seconds' duration, as the test taken was very malleable, and would not harden, the charge was cast into ingots without any addition whatever. The chemical composition of this charge and the results of the mechanical tests are as follows:

Blows.	C. Carbon by combustion.	Sulphur.	Phosphorus.	Manganese.	Breaking strain Tons.	Elongation in 10 in. Per cent.	Reduction of area. Per cent.
·748	——	·075	·030	·235	23·49	30·00	72·04
					23·49	28·25	70·01
					23·37	27·50	74·12

The heat as before gave no trouble in casting, and the steel was very malleable when worked at a high heat. Samples of the pieces submitted to tensile strains, together with bars plunged into cold water at a red heat, and afterwards twisted and bent cold, as well as ordinary cold bends, are exhibited.

A second charge was treated in exactly the same way, except that at the end of the after-blow 15 cwt. of white hematite pig-iron were added in lieu of spiegel. The resulting steel gave no trouble in casting, and an ingot, hammered to test its malleability, worked very well. Analysis of the steel produced gave the following results:

C. C.	Si.	S.	P.	Mn.
·55	Not estimated.	·107	·151	·612 per cent.

It will be noticed that the phosphorus and manganese are higher—the former than it ought to be, the latter than one would have expected to find, after the addition of a non-manganiferous pig; but as the charge of white hematite was melted in a cupola which had only just before contained a 20 per cent. spiegel charge, we think it likely that a small quantity of this must have remained behind and come out with the white iron. With respect to the phosphorus, the mixture was one with which we had little experience. It was also the last charge from the cupola, and, unfortunately, nearly a ton heavier than it should have been, which circumstance was not observed until it was poured into the ladle. And, although it was blown for 130 seconds after the disappearance of the carbon bands, exactly the case as blow 748, there is no doubt that it was underblown, as the metal, before the addition of the white iron, contained as follows:

S.	P.	Ma.
·125	·147	·360 per cent.

For some time past, except in the case of the first two or three blows from a new mixture, when samples have been taken to determine the length of the after-blow, we have blown to time, and all

testing during the operation has been dispensed with. It is true that our early results were not all satisfactory, but latterly since we have used a mixture containing about C.C. 4·0 per cent., Si ·5 per cent., S ·2 per cent., P 1·4 per cent., Mn 1·0 per cent. in the converter, we have succeeded very well, as the results below will show. During the week ending April 10th, twelve test pieces, taken at random from the week's work, contained of phosphorus as follows:

	Per cent.		Per cent.		Per cent.
No. 1,	·103	No. 5,	·065	No. 9,	·111
" 2,	·067	" 6,	·074	" 10,	·054
" 3,	·040	" 7,	·073	" 11,	·074
" 4,	·051	" 8,	·052	" 12,	·048

And in the week ending April 17th, when not a single sample was taken during the operation, except in the case of the experimental blow 748, the average amount of phosphorus contained in 36 blows, all of which were analyzed, was ·056 per cent., the highest being ·101 per cent., and the lowest ·019 per cent. The composition of this quality of steel has been in other respects very regular, the analyses and results of a test piece, 2 inches long and ·533 inches in diameter, being as follows:

Carbon.	Silicon.	Sulphur.	Phosphorus.	Manganese.	Breaking strain. Tons.	Elongation. Per cent.	Reduction of area. Per cent.
·40	—	·40	·085	·662	39·75	20·25	31·84

Since the sampling during the operation has been dispensed with there has been comparatively little trouble with the slag adhering to the sides of the converter, and the wear of the lining has been practically uniform. As many as eighty-seven blows, representing about 630 tons of steel, have been produced from one lining without any repairs whatever, except, after the fiftieth blow, new fireclay bricks to the nose. Thirty-seven more blows, equal to about 270 tons of steel, were converted in the same lining after renewing the front or blowing side and putting in a third fireclay nose. At the end of our last week's work (April 17th), during which bricks made of the best pot clay were used for this purpose, instead of being scoured away as had been invariably the case with the commoner ones, they were little the worse

for wear, and would, we feel sure, have run for a second week. The vessel bottoms (all made with a mixture of tar and lime rammed round pins) during this same week, averaged eight blows each, the maximum being twelve and the minimum four; but this bottom was taken out on completion of the week's work, and was very little worn. With respect to output, during the eight weeks ending April 17th (omitting Easter week, which was a broken one), 3380 tons of steel were made, or an average of 422 tons per week, the largest week being that ending March 27th, when 541 tons 7 cwt. were produced. In conclusion, we think it will be apparent to all that there are no difficulties in the working of the process. We are satisfied that as good steel can be produced by it from phosphoric pig, and quite as regularly as that obtained from hematite in the ordinary gannister-lined converter. We have shown that it is an easy matter to produce a malleable ingot iron, containing practically no carbon and very little manganese by this method, which, to say the least, it is rather difficult to do from hematite pig-iron by the old process; and although our only experiment (results of which we have laid before you) to produce a harder quality of steel from phosphoric pig high in manganese, without the addition of any manganiferous pig, at the end of the operation did not turn out as satisfactorily as we could have wished, there appears to us little doubt but that this too will be accomplished, as well as the production of soft steels, suitable for boilers and ships' plates, etc., in a similar manner. It is true that our production from one pair of converters has never yet exceeded 541 tons 7 cwt. in one week, but it must not be forgotten that the process is still a new one, and that the plant at our disposal is not of modern construction, nor well adapted to its requirements. These requirements are, in our opinion (on account of the lining), either additional fixed converters, or duplicates with proper facilities for changing, as well as suitable arrangements for the speedy removal of the large quantity of slag. Under these conditions, in a well-arranged shop, we feel sure that not only as great an output can be obtained by the process as is now being produced in the best English practice in the conversion of hematite, but also that it may be made to equal anything that has ever been accomplished by our friends on the other side of the Atlantic.

ON THE STEEL-COMPRESSING ARRANGEMENTS AT THE BARROW WORKS.

By Alfred Davis, of London.

Paper read before the Institution of Mechanical Engineers, at Barrow.

The unsoundness of steel castings, particularly in the case of ingots made by the Bessemer or Siemens-Martin process, has given manufacturers considerable trouble, and occasioned much waste of material.

A good deal has been stated and written of late as to the cause of this unsoundness, which occurs principally at the upper end of the ingot; but it appears now to be pretty generally conceded that the defects proceed from two distinct causes: First, the existence of gases, generated at the point of transition from the fluid to the solid state, which are imprisoned in the form of bubbles when the surrounding metal becomes solid; and secondly, the existence of spaces formed by the natural contraction of the metal in cooling, by reason of the outer skin first becoming solid, and refusing to follow up the interior portion of the ingot, which subsequently cools, and consequently occupies a smaller space.

Various systems, designed to cure this evil, have already been discussed before this Institution. The system of compressing fluid steel by the direct application of high pressure steam, has recently been adopted by the Barrow Hematite Steel Works, and by Messrs. Bolckow, Vaughan & Co., and has the merit of simplicity combined with efficiency. The arrangements adopted for the purpose are founded upon those used by Mr. H. R. Jones of the Edgar Thomson Steel Works at Pittsburgh, where the system has been used for some years.

A high pressure steam boiler is provided, and communicates with a receiver, which is attached to the side of the ingot crane, and which is furnished with a row of cocks corresponding with the number of ingot moulds. From these cocks strong india-rubber pipes convey the steam to the ingot moulds, which are arranged in the arc of a circle round the ladle crane. The metal from the ladle is poured through a loose pouring cup, which rests on a conical seat at the top of the ingot mould. As soon as the pouring is finished, this cup is removed, and a lid having the steam pipe ready coupled to it, is placed on the top of

the mould, and secured to it by a steel cover. The cock on the receiver is then opened and the steam allowed to act upon the metal until it has completely set. The result of this pressure is to make the ingot sensibly shorter than when cast in the ordinary manner, the difference, according to experiments made at the Edgar Thomson Works, being from 1½ inch to 2 inches in a 5 feet or 6 feet ingot. The ingots when cold are perfectly level at the top, and there is no porous head requiring to be cut off.

The arrangements adopted by the Barrow Steel Company differ somewhat from those in operation at the Edgar Thomson Works. These arrangements require only a very brief explanation.

The ingot moulds, which are of similar construction to those used by the Edgar Thomson Company, are placed in a row, within a dock or siding, the centre line of which runs to the centre of the pit. The metal flows from the ladle into a trough mounted upon wheels and provided with runners at points corresponding with the centres of the ingot moulds when the trough is in position. This trough runs upon rails, placed on either side of the row of ingot moulds, and can readily be removed after the moulds are charged. Each mould is provided with a steam-tight cover, having a wrought-iron pipe attached to it, furnished with a stop-cock. This pipe communicates at right angles with the main steam pipe, which runs parallel with the side of the dock. The junction of the branch steam pipes with the main is formed by means of a cast-iron sleeve piece, with stuffing boxes, to enable the covers, with their respective cocks and pipes, to be thrown back out of the way when not in use.

The boiler for supplying the steam has been constructed by Messrs. Daniel Adamson & Co. It is 3 feet 6 inches in diameter and 9 feet high, and is intended to be worked at a pressure of 200 pounds per square inch.

At the Cambria Steel Works in Pennsylvania an attempt was made two or three years ago to inject water through the cover of the ingot mould after the metal had been poured. The heat of the molten steel of course generated steam, which acted as a compressing medium; a safety valve being provided and loaded to the pressure required. The disadvantages of this system, as compared with that now described, are sufficiently obvious; the complication of parts and the danger from explosions being very great.

The results obtained by the process of casting ingots under steam

compression are highly satisfactory. Not merely is the ingot perfectly sound, but the action of the steam is such as to enable the men to work it earlier and in a hotter state than with the ordinary method, so that there is an appreciable increase in the output. The presence of the steam also acts beneficially on the sides of the mould, and causes it to last longer.

The pressure necessary to produce a perfectly sound ingot will depend upon the quality of steel to which it is applied. At the Edgar Thomson Works it is found that for ordinary rail metal 100 pounds per square inch is sufficient. But for milder steel a higher pressure is needed; and since experience has proved that steam is readily dealt with at very high pressures, there does not appear to be any reason why 1000 pounds or 1500 pounds per square inch should not be applied if required. It is only a question of giving sufficient strength to those parts which are exposed to the pressure. As a matter of fact the boilers designed by Mr. Loftus Perkins will carry a steam pressure of 2000 pounds per square inch with perfect safety. The question of making tight joints between the ingot moulds and covers with such high pressure is one of considerable importance; but there are several ways in which this difficulty may be overcome. In using steam at a very high pressure, the size of the supply pipe may be considerably reduced, and the mode of attachment greatly simplified; and since the amount of steam used is inconsiderable, the size of the boiler would be correspondingly small. As an alternative, in cases where high pressures are needed for the consolidation of fluid metals, the author proposes the use of compressed air. With this system a pressure up to 1500 pounds or 2000 pounds per square inch may be obtained without danger or difficulty, as is completely demonstrated by the torpedo practice at Woolwich, and by the experiments carried out by Colonel Beaumont, in connection with the use of compressed air for tramway locomotion.

The advantages of an elastic compressing medium in the consolidation of fluid metals, as compared with the hydraulic process, scarcely need to be dwelt upon. In applying hydraulic pressure a rigid piston is necessary; and the outer portions of the cooling mass (which are the first to set) must be crushed down, before the interior portions, which are still liquid, are reached by the pressure. A considerable amount of power is wasted in consequence. In addition the fluid metal is forced against the sides of the mould, and in a contrary direc-

tion to that which it naturally follows in the operation of cooling. With steam or compressed air the operation is reversed; as soon as contraction commences the entire ingot is surrounded by a uniform pressure, which continually follows up the natural contraction of the mass.

In conclusion, the author would suggest that the principle of elastic pressure, in connection with the consolidation of fluid metals, although at present applied to Bessemer ingots only, is well worth the consideration of those interested in the manufacture of all kinds of steel and iron castings, and particularly of heavy guns.

The discussion on Mr. Davis' paper was opened by Mr. E. Windsor Richards, who referred to experiments which he had made at the Eston Works, and who showed photographs of ingots which had been planed down the middle to ascertain whether or not they were sound. In examining ingots it was, he maintained, necessary to plane them apart in this way in order to expose their soundness or unsoundness throughout their whole depth. Of the ingots shown in the photographs one had not been compressed, while No. 2 had been compressed with steam at a pressure of 80 pounds, and No. 3 with a pressure of 130 pounds. These sections showed a progressive improvement, but bubbles were still visible in No. 3, this showing that a still higher pressure of steam was desirable. To supply steam at this increased pressure a small vertical boiler has been constructed by Mr. Daniel Adamson for use at the Eston Works, while a similar boiler is being used at Barrow, it being worked at 200 pounds per square inch. With this pressure Mr. Richards hoped to get a perfectly solid ingot. Mr. Richards next dealt with the gases contained in the bubbles or honeycombs. It was the custom in former days, he remarked, for forge managers when anything went wrong to blame it to the presence of sulphur; in the same way it was now the fashion amongst steelmakers to attribute mishaps to carbonic oxide. Experiments which he had made, however, disposed of the theory that the gas enclosed in bubbles formed in an ingot contained only carbonic oxide, and showed that the gases really enclosed were chiefly hydrogen and nitrogen. To prove this fact he had had a tank made sufficiently large to contain an ingot, this tank having a stuffing box at the bottom through which a drill could be introduced. The ingot to be experimented with was placed in the tank, the latter being filled with water, and a hole 2½ inches in

diameter was then drilled up 4½ inches into the bottom of the ingot. This drilling resulted in the release of gases, and the gases so evolved were collected and analyzed by Mr. Stead, of Middlesbrough, when the composition was found to be as follows: Hydrogen, 78·6 per cent.; nitrogen, 20·4 per cent.; carbonic acid, 0·2 per cent.; and carbonic oxide, 0·8 per cent. From the hole just mentioned, no less than 18 cubic inches of these mixed gases were obtained. Altogether he considered the process of compressing fluid steel was a most valuable and essential one. In answer to a question put by Mr. Cowper, he also stated that he considered the gases were, by the application of steam, expelled through the openings left at the bottom of the mould.

The next speaker was Dr. Siemens, who remarked that the benefits to be derived from the compression of fluid steel were well proved by the results obtained by Sir Joseph Whitworth. He (Dr. Siemens) had himself tried to compress fluid steel some time ago, but he had not succeeded in devising a simple means of carrying out the process. He was therefore much interested in the system of steam compressing when it was brought before the Iron and Steel Institute, and had offered to give facilities for its trial. He found, however, that with very mild steels the steam pressure available was too low to insure satisfactory results. Sir Joseph Whitworth employed a pressure of two tons per square inch. With mild steels they were much troubled with honeycombing near the surface of an ingot, and to remove such honeycombing a pressure of one to two tons per square inch appeared essential, although central cavities might be got rid of with moderate pressures. Where the gases went to during the process of compression was somewhat difficult to determine, but he believed that the gases were simply reduced in volume, and occluded. Referring next to the failure of the *Livadia's* boilers, Dr. Siemens stated that he had received samples of the steel, and had had it analyzed. Inasmuch, however, as the matter was put before him in a semi-official way, he did not feel at liberty at present to lay details of the analysis before the meeting. It was, however, especially desirable that the failure of the *Livadia's* boilers should not be regarded as impugning the character of good mild steel. A boiler made of such steel could not, he contended, be burst under any circumstances, as the metal would stretch at the rivet holes, and the boiler would foul by leakage, the rivet holes being drawn oval. The experiments made by Mr. Deane, at Swindon, and described by Mr. Parker in a paper read before the Institute of Naval

Architects proved this, while Messrs. Eastons and Anderson had also tried unsuccessfully to burst a steel boiler made by them, this boiler being made red hot in a special furnace, and again tested after recaulking where necessary. Altogether, mild steel was the most trustworthy material known, and it might be punched, sheared, etc., without injury. The experience of the Admiralty with the steel used in the *Iris* and the *Mercury* showed this: this steel was supplied by works in which he was interested, and not a single plate or angle iron from these vessels having been returned to the makers. Since then the Admiralty had employed steel for both ships and boilers almost to the exclusion of wrought iron, about 10,000 tons having been supplied to them. The "demon of cheapness" was, however, abroad, and was exercising a most detrimental influence on steel plates. The works he had already alluded to were now making two qualities of steel plates, one being a cheap class of steel, while the others were of a special class of metal, the quality of which they guaranteed, and in the manufacture of which exceptional care was taken. Notwithstanding the advantage of this latter class of steel, many engineers preferred the cheaper variety even for boiler work, although if they were using iron they would pay for Lowmoor at a rate far higher than that of the best steel. Much of the rivet steel which is being sold is also far from being of the quality which was desirable, the rivet bars being in some cases rolled from scrap ends of steel of a quality by no means suitable. Taking these circumstances into account, it was not fair to criticise the behavior of any particular steel plate or bar without specifying the price paid for them.

The next speaker was Mr. Thomas Adams, who stated that he had been employed by the Board of Trade to carry out for them experiments on different samples of steel. The officials of the Board had previously made experiments at Liverpool, but had been unable to burst the vessels owing to leakage, the highest pressure they succeeded in attaining being 500 pounds per square inch. They then made trials at Glasgow, when they only succeeded in getting 480 pounds. Returning to Liverpool they again got 500 pounds, while at some railway works they got up to 750 pounds, and at London reached 1500 pounds per square inch, the latter pressure however, being unable to burst a $\frac{5}{16}$ inch plate. After these failures they went to Manchester and placed the matter in the hands of Mr. Adams, who succeeded, he stated, in making a vessel which stood 2000 pounds per square inch without

leaking, and which failed through the fracture of the plate. The testing vessel made by Mr. Adams consisted of a strong rectangular frame like the bottom ring of a locomotive firebox, this ring having a thick plate placed on one side of it, and the experimental plate on the other. To obtain tightness of joints two strands of twine were laid between the plates and frame inside the line of rivets, and two similar strands outside that line. Besides being secured to the frame around their circumference the plates were of course stayed together, great care being taken with the fit of these stays, and they being provided with nuts at the ends. With a test vessel so made Mr. Adams informed the meeting that the joints kept perfectly tight, and it was only necessary to employ a single plunger one inch in diameter by six inches stroke to carry out the hydraulic test. In reply to Mr. Crampton, Mr. Adams stated that he had not employed a cylindrical vessel, as the joints could not be kept tight.

Next, Mr. Snelus remarked that Mr. Richards was evidently not very positive as to what became of the gas when it disappeared under the application of pressure to fluid steel. He quite agreed with Mr. Richards that some delusion had prevailed as to the nature of the gases contained in the bubbles in an ingot, and this had in a great measure arisen from the conclusions experimentally arrived at by Sir Henry Bessemer, to the effect that an ingot during the process of setting gave off carbonic oxide. He (Mr. Snelus) had many years ago read a paper before the Iron and Steel Institute "On the Gases evolved during the Bessemer Process," and he then showed that during the earlier part of the blow there is discharged an excess of carbonic acid, but ultimately an excess of carbonic oxide. With regard to the occlusion of gases, palladium occludes about 700 times its volume of hydrogen, while molten silver occludes oxygen, to a remarkable extent, but gives it up during setting. Thus certain metals appear to have a special affinity for particular gases, and it was quite possible that steel might have such an affinity for carbonic oxide, this gas being occluded throughout the metal, while cavities were filled with hydrogen and nitrogen. Taking all these matters into account there was probably a mode of getting rid of the blowholes by chemical means instead of by compression, the idea being to mingle with the steel some body which occludes the gases which at present collect in bubbles. Altogether he did not believe that the gases were driven out by compression, but simply that the compression induced more powerful occlusion, this

practically being the theory which had been advanced by Professor Müller, of Osnabrück.

Mr. Hayes next suggested that probably the compression by the admission of steam might be more effectively performed if the steam were admitted at both ends, and Mr. Sharp stated that he had cast copper perfectly solid by employing a secret chemical means of preventing the formation of blowholes. It was then announced by Mr. Cowper that Mr. J. T. Smith had at his suggestion kindly made a small steam boiler, which it was intended to burst by hydraulic pressure at the steel works that afternoon.

The next speaker was Mr. Arthur Paget, who asked Mr. Davis if the process described in the paper had not some twelve months prior to its use in the United States, or in England, been described in a paper read before a scientific society in France, and also whether a similar process, but using air in place of steam, was not in use at the Bolton Iron and Steel Works. Mr. Paget further inquired whether there was really any advantage—as suggested in the paper—in employing an elastic compressing medium in place of a plunger. Next, Mr. R. E. Crompton directed attention to the proportions of hydrogen and nitrogen mentioned by Mr. Richards as existing in the honeycombs, and were they such as to suggest the formation of ammonia, which would of course be affected by the moisture in the steam. Mr. Crompton further mentioned the experiments of Edison as appearing to show that the entire removal of occluded gas from metals deprived them of their ductility. Mr. R. H. Tweddell next asked if Mr. Richards could give any data as to how the specific gravity of the steel was affected by the compression, but unfortunately this inquiry was not replied to.

Mr. Davis, in replying to the discussion, remarked that he considered that the photographs exhibited by Mr. Richards proved the value of the process he had described, while he believed that a pressure of 200 pounds per square inch would be found sufficient for dealing successfully with any class of steel. It was, however, most important to apply the pressure directly after pouring, and by means of the new appliances at the Barrow Works it had been rendered possible to apply the pressure in 13 seconds after teaming. He was quite aware of the French trials which had been made with this system of compressing by M. Considore, and he had inserted a paragraph concerning them in his paper, but it had been struck out. In reply to a letter on this

subject signed "Justice," which had appeared in *Engineering*, he had received a communication from Mr. Jones, of the Edgar Thomson Steel Works, in which he stated that he (Mr. Jones) had made his first experiments on the use of steam in this way in 1875. The fact was that the same idea had evidently occurred independently to Mr. Jones and Mr. Considore. Mr. Davis then proceeded to explain a variety of modes of fixing down the mould in which ingots were to be compressed, and he concluded by remarking that he was unaware of the use at Bolton of air under pressure for compressing fluid steel.

ON THE MANUFACTURE OF JUTE.

By WILLIAM FLEMING, of Barrow-in-Furness.

Paper read before the Institution of Mechanical Engineers, at Barrow.

While jute has long been known to the natives of Bengal, and largely used by them in various textile manufactures and for paper-making purposes, its introduction into England is of comparatavely recent date.

In 1796 the East India Company imported some small quantities of jute, and afterwards continued importing it in small lots now and then, but it made no progress whatever with the manufacturers of this country. What was thus imported seems to have been employed in the neighborhood of London in the production of door mats, ropes, etc. Portions of the samples, however, appear to have found their way to Abingdon, in Oxfordshire, where there were a few manufacturers of sackings and woollen carpetings. There it was spun, by hand, and used up to a small extent in some of their fabrics. The Abingdon manufacturers therefore appear to have the credit of being the first to employ jute in textile fabrics in this country.

About 1833 some of the jute yarn thus spun at Abingdon was sent to Dundee, where the matter attracted attention, and shortly afterwards was commenced at Dundee that manufacture of jute, which has resulted in such an extraordinary development of this industry in Great Britain, Ireland and the Continent. The increase in the consumption of jute during the last fifty years is most remarkable. The total export of jute from Calcutta in 1829–30 amounted to 20 tons, valued at 60*l.*; it has now risen (in addition to the enormous consumption for manufac-

turing purposes in Bengal itself) to upwards of 350,000 tons, or nearly 2,000,000 bales annually, amounting in value to about 6,000,000*l.*

Jute is mainly grown in Bengal and exported from Calcutta. It is sown in March and April and during the following three months attains a height of from 10 to 12 feet, while the stems reach from one to two inches in circumference.

Shortly after the plant has flowered it is cut down near to the ground, tied up in bundles of from 50 to 100 plants each and "petted," that is, steeped in stagnant water from eight to ten days till the "bast" (or the fibre lying between the bark and the stem) can be separated from the wood. It is then removed from the water and beaten gently against a board until, with a little management, the native operator can strip off the whole from the stem without damage to either stem or fibre. The fibre is then drawn through the water until all impurities are washed or picked off. It is then dried in the sun, and, after having been assorted into different grades of quality, is exported, under various distinctive marks, in bales of 400 pounds each to London, Liverpool, Dundee, Barrow and other markets.

Softening.—The jute fibre being of a somewhat harsh nature, the first process which it has to undergo after being released from the bale, where it is very tightly compressed, is that of softening. This is done by dividing the jute into stricks or handfuls, and passing these stricks through between a series of heavy fluted rollers, which, by crimping and crushing, and in a manner rubbing the fibres, render them softer and more yielding.

The softening machine consists of four horizontal rows of fluted rollers about 9 inches in diameter and 2 feet 6 inches long, and ten rollers in each row. Each roller in the bottom or fourth row bears the weight of the three rollers above it, those in the third row are pressed by the weight of two rollers, and those in the second row by the weight of one roller. The stricks of jute pass first between the pairs of rollers constituting the first and second rows, then return between the second and third rows, and pass lastly between the third and fourth rows, being delivered in a softened condition at the opposite end of the machine to that where they were introduced, and having been subjected during the process to an increasing weight as they entered between the different rows. The softening of the jute is at the same time assisted by an operation called "batching," *i. e.*, the sprinkling of the stricks with oil and water whilst they are passing through the machine. This

is done by having two cisterns, one containing water and the other containing oil, placed over the machine. Inside the cisterns are revolving rollers which lift the liquids and discharge them over a scraper, or doctor, dropping them upon the jute as it passes between the rollers.

Breaker-Card.—The jute having been softened, and weighed into bundles, is conveyed to the breaker-card.

The principal parts of this machine consist of a cylinder 4 feet in diameter by 6 feet wide on its working surface, covered with sharp steel pins inclined slightly forward in the direction in which the cylinder revolves, and of a number of small rollers arranged round the periphery of the cylinder, each of these rollers being also covered with pointed pins. The stricks of jute are laid or spread by the attendant upon an endless traveling sheet, which conveys them to the first roller, called the feeder, and the pins which cover the surface of this roller enter the jute and carry it forward to the point where it comes in contact with the pins of the cylinder. The surface speed of the pins of the feeder being about 10 feet per minute, whilst the speed of the pins of the cylinder is about 2,000 feet per minute, the fibres, which are slowly presented and delivered by the feeder, receive a severe combing or dressing from the pins on the cylinder before they are finally released by the feeder. The cast-iron shell which encases the feeder for about one-sixth of its circumference serves to keep the fibres imbedded in the feeder-pins, so as to prevent their being carried off by the cylinder before being properly carded. A large quantity of the fibre, however, when struck by the pins of the rapidly revolving cylinder, is broken off and carried forward on the points of the cylinder pins, and it is to straighten out, comb and split these portions that the other rollers, called workers and strippers, are applied.

The first roller that acts, after the fibres have left the feed roller, is the worker. This roller, which is about 9 inches in diameter, is placed with its pin-points at a distance of from $\frac{1}{16}$ to $\frac{1}{8}$ inch from the points of the cylinder pins. The angle of the worker pins is very much more acute than that of the cylinder pins, and inclined in the opposite direction. The roller revolves in the same direction as the cylinder, but at a speed of only about 50 feet per minute. The effect is that the fibres projecting from the pins of the cylinder are caught on the pins of the slowly revolving worker, and as the direction and pull of the cylinder pins tend to force the fibres on to the pins of the worker, a

considerable portion is retained by the latter. The worker is in its turn cleared of fibre by the stripper, a roller about 13 inches in diameter, which revolves at a speed of about 430 feet per minute in the opposite direction to the worker, and, traveling with pins inclined forward, it strips the fibres from the worker and is afterwards itself cleared by the still more rapidly traveling cylinder.

The same process is repeated at the second worker and stripper, which are placed rather closer to the pins of the cylinder than the first two rollers. After passing the second worker and stripper, the fibres are carried forward to the doffer, a roller about 16 inches in diameter, which travels in the same direction and at about the same speed as the worker, and has its covering similarly arranged, except that in the doffer the pins are rather finer and more numerous. The pins of this roller are set close to those of the cylinder, so that the whole of the fibres are caught by them and carried round to the two doffing-rollers, which take the jute from the doffer in the form of a continuous broad sheet or fleece. This thin sheet of carded jute, after it issues from the doffing roller, is gathered together or contracted from about 5 feet to 4 inches by means of a tin conductor, and it then passes through the delivery-rollers in the form of a continuous sliver, and falls into a can.

The surface speed of the doffing and delivery-rollers is generally about fourteen times quicker than that of the feed-roller; consequently if the jute be spread so that the feed-roller receives about 2 pounds per yard, the sliver delivered into the can will measure about 7 yards per pound.

The tin rollers under the workers and strippers are to prevent the fibres, as much as possible, from falling to the ground when the stripper is clearing the worker.

Finisher-Card.—The jute sliver as delivered by the breaker card is not considered to be sufficiently carded for most purposes, and it is therefore necessary that the process of carding should be repeated on a second machine called the finisher-card. The principle on which this machine works is exactly the same as that of the breaker card, but instead of only two workers and two strippers there are sometimes three, four or five pairs of these rollers, and the pins on the surface of the cylinder and rollers are finer and set closer to one another, so as to comb and split the fibres more efficiently. The finisher-card is fed by slivers from twelve cans from the breaker card upon an endless traveling sheet, similar to that used in the breaker-card, which carries them

forward to be acted upon by the cylinder of the finisher in the same manner as the stricks of jute are acted upon in the breaker-card. It will be understood that the slivers as delivered by the breaker-card, although continuous, must necessarily be rather irregular, *i. e.*, thicker in some parts than others, but by putting twelve of these already partially carded slivers through the finisher together, a kind of average is struck and the slivers delivered by the finisher card are much more regular and even. The "draft," or proportion of speed between the delivery roller and feeder of the finisher-card is about 16 to 1, so that, being fed by twelve slivers, each measuring about 7 yards per pound, and these being subjected to draft of 16, the sliver delivered by the finisher-card will measure about 9¾ yards per pound.

Drawing-Frame.—The next process after carding is to have the fibres of jute drawn out straight and laid parallel alongside one another, and this is accomplished on a machine called the drawing-frame.

There are several kinds of drawing frame, all intended to produce the same results, but the kind most in use is the system called the "spiral gill drawing-frame." In this machine the slivers, which have been delivered into the cans from the finisher card, pass over a conductor plate and thence between three rollers, which are called the retaining-rollers, and are in fact the feed-rollers of the machine. Just in front of the delivery side of these rollers is a series of traveling bars on which are fixed hackles or gills, *i. e.*, brass stocks with steel pins standing upright in them. These bars, with the gills attached, travel forward from the retaining-rollers, carrying with them the jute fibres into which the pins penetrate, their speed being the same as that of the retaining-rollers, or just as much faster as will insure that the slivers are kept tight and do not rise off the pins. The bars are propelled by means of two longitudinal screws, one at each end of the bar, cut at a pitch varying from 1½ to 2 threads per inch. The end of the bar which enters into the thread of the screw is beveled to the angle of the thread, so that the body of the bar and the pins are quite vertical, whilst the end fits the spiral, and there being one bar in each thread of the screw, when the screws revolve the bars glide forward, supported on steel slides. These screws are called the top screws, and the length of their threaded part is made suitable for the length of fibre, say about 10 or 11 inches for carded jute. As each bar arrives at the front or further end of the top screws it drops from the slide

which has been supporting it in position into the threads of two bottom screws placed exactly under the top screws, cut to the same hand, but revolving in a contrary direction. These bottom screws accordingly carry back the bar, supported on bottom slides, and with its pins still upright, to the other end of the frame, where the thread of each screw is terminated by a projecting cam, by which the bar is lifted up into the top screw again, close to the retaining roller, causing the pins of the gills to penetrate the sliver which the retaining roller is delivering. Thus a continuous forward travel of bars is maintained for 10 or 11 inches in length in front of the retaining rollers, moving at what is practically the same speed.

At the front or delivering end of the screws, where the bar drops out of the sliver, are the drawing-roller and the pressing-roller above it, the former being made of steel, about $2\frac{1}{2}$ inches in diameter, and the latter generally of cast-iron covered with leather about 8 inches in diameter. These rollers, which are pressed together by means of weighted levers, move at a speed about six or seven times greater than the retaining-rollers or gills; consequently the fibres, when delivered by the gills as the traveling bars drop from the top to the bottom screws, are seized between the rollers and drawn away from the pins of the gills, which act as a kind of comb, holding them back and insuring that they do not enter the bite of the rollers in a crossed or tangled state.

The length of each traveling bar is about 3 feet, and there are fixed on it four gills, each 6 inches wide on the pins. Each set of bars and gills, with its retaining and drawing-rollers, conductors, etc., constitutes what is called a carriage; and drawing-frames are made with one, two, three, four or five carriages. Thus a drawing-frame of two carriages, and four gills per carriage, will have eight sets of gills, and as two slivers from the finisher-card are supplied to each gill, the number of cans at the back of the machine will be sixteen. Assuming the speed of the drawing-roller to be six times that of the retaining or feeding-rollers and gills, it is evident that the sliver delivered by the drawing-roller will be six times longer than when it enters the machine, and as there are two card-slivers to each gill, and each card-sliver may be assumed to measure $9\frac{1}{2}$ yards per pound, the sliver coming from between the drawing and pressing-rollers will measure about 28 yards per pound. But, for the purpose of completing the process of straightening the fibres and laying them parallel, as well as more

effectually equalizing the slivers, it is necessary to put the jute through a second drawing-frame, and, therefore, it is more convenient to unite the slivers from two gills upon a doubling-plate arranged for the purpose in front of the drawing-roller, and so to deliver the sliver from all the eight gills comprised in the two carriages through four pairs of rollers, called delivery-rollers, into four cans; the sliver in each can will consequently measure about 14 yards per pound.

The second drawing-frame is constructed in an exactly similar manner to the first drawing-frame, except that the pins in the gills are rather finer and ranged closer together, and instead of the slivers from two gills being united together in front of the drawing-roller, the slivers from each gill are carried straight from the drawing to the delivery-rollers and run into the can, thus making eight deliveries from the machine. As there are two slivers, each of 14 yards per pound, put up to each gill, the sliver as delivered into the can from this second drawing-frame, if the draft on the machine is six, will measure 42 yards per pound.

Roving-Frame.—The next machine in the system of preparing machinery is the roving-frame. The object of this machine is to draw out still further the fibres in the jute sliver and wind them on bobbins in a form convenient for the final process of spinning into yarn. The manner of drawing out the slivers is exactly the same as that employed in the drawing-frames, but as only one sliver is put up to each gill in the roving-frame, and that sliver measures about 42 yards per pound, narrower and smaller gills serve the purpose, and eight gills can be fixed on each bar instead of four, and in consequence of the slivers being so much lighter, the pins of the gills are finer and set closer together. The draft of the roving-frame is usually about seven, so that the sliver delivered by the drawing-roller of the roving-frames will measure about 294 yards per pound.

This is too thin a sliver to deliver into a can, and for that reason, and also for general convenience, the sliver is twisted slightly and wound on to a bobbin; in this form it is called "rove." In order to twist the sliver into rove and wind it on to the bobbin, there are employed a spindle and flyer, constructed on the same principle as in similar machines for cotton and other fibres, but for jute the parts are larger and stronger. The number of spindles in a jute roving-frame may vary from twenty-four to sixty-four; a very usual number to put in is fifty-six, and as there must be a gill for each spindle and flyer, seven

carriages with eight gills on each bar are necessary to supply fifty-six spindles. The spindles with their flyers stand vertically in two rows in front of the drawing-rollers and revolve at a speed of about 600 revolutions per minute. Inside the flyer, and turning freely on the spindle, is placed the bobbin which is to receive the rove. This bobbin is driven by gearing independently of the spindle and flyer, but revolves in the same direction. The amount of twist given to the rove should be only just sufficient to give it strength to unwind from the bobbin, in the subsequent process, without allowing the fibres to separate. Now, suppose the suitable twist for rove to be one turn per inch of sliver delivered; then the speed of the drawing-roller must be arranged so as to deliver one inch for each revolution of the flyer, and the speed of the bobbin must be calculated to be just sufficiently behind that of the flyer to take up the quantity delivered by the drawing-rollers. For instance, the spindle and flyer make 600 revolutions per minute, the roller delivers 600 inches per minute, and the circumference of the bobbin shank is 5 inches, the speed of the bobbin will have to be 480 revolutions per minute, or 120 less than the flyer, because by lagging behind to that extent the bobbin will wind up 120 laps, each 5 inches long, or 600 inches per minute. But, as the bobbin fills, the diameter on which it winds continually increases, and the speed must be diminished accordingly. Thus, when the bobbin is nearly full, its circumference will be 15 inches, or three times what it was at starting; consequently the bobbin will have to run only 40 revolutions per minute behind the flyer in order to wind the rove with one twist per inch. In order to wind the rove regularly from top to bottom of the bobbins, the rail on which these are carried is made to rise and fall, so that the bobbin moves up and down inside the legs of the flyer, and the diminution in speed of the bobbin is caused by a projection attached to the lifting-rail being arranged to release a catch each time that the rail arrives at top and bottom of its traverse. The regulating motion is obtained from a bowl or pulley with a leather face, which is made to revolve by contact between two flat circular iron discs rotating in contrary directions. Each time that a catch is released either at top or bottom of traverse, the bowl is allowed to slide a little nearer to the centre of the two discs, whereby its speed is diminished, and in this way the speed of the bobbin is also reduced for each successive lap that is laid on.

As already explained, the jute fibres in this slightly twisted form

are termed "rove." But for the heavier classes of yarn (say when 1 pound measures less than 400 yards), it is very usual to make the "yarn" on the roving-frame. To do this it is only necessary to increase the twist sufficiently to give proper strength to the yarn.

Spinning-Frame.—The object of this machine, which completes the first stage in jute manufacturing, is to reduce the rove which comes from the previous machine to the required size and then to twist the fibres together so as to form what is known as "yarn."

The number of spindles in a spinning-frame varies according to circumstances, but for an average class of jute yarns, a very usual number is sixty-four on a side, arranged in one row, at a pitch or distance from one another of 3¾ inches. The main parts of the machine are: the rack or creel for the bobbins containing the rove, the retaining-rollers, the binding-plate, conductor, drawing-rollers, thread-plate, spindle and flyer and bobbins to receive the yarn. The bobbins containing the rove are placed on pins fixed on the rack, and the rove is introduced through a conducting-plate between two fluted rollers pressed together, which form the retaining-rollers; then over the binding-plate and through a narrow conductor into the bite of the drawing-roller and pressing-roller, the former being made of iron and the latter of wood and pressed against the drawing-roller by means of steel springs. The drawing and pressing-rollers run at a surface speed considerably in excess of the retaining-rollers, the difference being regulated by change-wheels to suit the size of yarn required. We will assume the drawing-roller to be moving seven times faster than the retaining-roller, so as to give a draft of seven. The slight twist which the rove has received in the roving-frame gives it sufficient consistency to hold together and pull the bobbin round on its pin, so as to supply the retaining-rollers, but when gripped at one end between the retaining-roller and its pressing-roller, and at the other end between the drawing and pressing-rollers, the latter pair moving at the quicker speed, the slight twist which the fibres have received does not prevent them from parting. Not being supported by gills, as in the drawing and roving-frames, the rove is made to press against the binding-plate in order to retain the twist, and so prevent the fibres from separating too easily and from passing through the drawing and pressing-rollers in an irregular manner.

By the draft of seven, the rove is elongated from 294 yards per pound as it enters the retaining-rollers to 2058 yards per pound when

it passes from the drawing-roller. The fibres are twisted by the spindle and flyer in the same manner as in the roving-frame, but the amount of twist given to the yarn is very much greater than that given to the rove, and the bobbin is not, as in the roving-frame, driven by wheels, but is dragged round after the flyer by the thread and is retarded by friction sufficiently to wind on the yarn, the friction being regulated by the attendant to suit the strength of the yarn.

In illustrating the action of these different machines, a certain weight of jute has been assumed to be laid on the feed-sheet of the breaker-card with certain drafts and doublings in the separate subsequent machine; the result of which would be to produce yarn known as "7 pound," because each "spindle," or length of 14,400 yards, would weigh 7 pounds, but by varying the weight, drafts or doublings other sizes of yarn are produced. The finest yarns in jute, made from the best qualities, are about $1\frac{3}{4}$ pounds to 2 pounds per spindle, or say about $\frac{1}{4}$ pound per mile, whilst from the coarsest class yarn is made of which a mile will weigh more than 30 pounds.

The yarns, having been prepared in the necessary forms for the looms, are woven into fabrics of great variety, suitable for the requirements of every market in the world, and these fabrics undergo various processes of calendering, mangling and finishing.

Jute is the cheapest fibre known, and hence the very general demand for jute fabrics in every country. Jute manufactures are now almost entirely used for the conveyance of grain, flour, rice, linseed, coffee, pepper, saltpetre, etc., as also for guano and all chemical manures, while in the export of the raw materials, cotton and wool, nothing else is now employed for packing. All makers of textile goods now use jute hessians and baggings for the packing of their manufactures.

The finer qualities of jute yarns are woven into fabrics suitable for the production of curtain cloths, tapestries, etc.; for furniture purposes (such as the "Kalameit," now produced in the Barrow Flax and Jute Works) and for carpets, rugs, etc. They are also used largely, in combination with cotton, silk and woolen yarns, in the weaving of numerous ornamental goods. In fact, the list of the varied purposes to which jute, jute yarns and jute fabrics are now extensively applied is curious and remarkable, embracing, as it does, telegraph cables, wire ropes, oilcloth and linoleum manufactures, ropes, twines, cords, etc., even down to artificial hair.

The cuttings (or the few inches of hard fibre cut from the bottom of

the plant in India) are now largely used by the paper-makers in this country, as well as in India, America and the Continent, and the wastes made in the general manufacture of jute, which cannot be spun over again, come into value in connection with paper-making, felt-making and other purposes.

This enormous and general demand has brought about a more than equivalent producing power in this country, in India and elsewhere, and during the past few years the jute trade has been suffering from the effects of this over-production, but as the requirements of the world are increasing so rapidly, the improved demand must soon rectify this unfortunate state of things.

DIRECT MEASUREMENT OF THE INTERNAL RESISTANCE OF MAGNETO-ELECTRIC MACHINES IN MOTION.

By TH. DU MONCEL.

In a series of articles Mr. G. Cabanellas has recapitulated the means that can be employed to investigate in an easy manner the best method of working, and the current yielded by magneto-electric machines, and to allow of their being classified in the order of their merit.

One of the greatest difficulties he has encountered has been the determination of the internal resistance of the generators, with the values that result from effects produced inside the generator while in motion, and which play an important part in the formulæ deducted from the laws of Ohm and Joule. It has been known for a long time, from the researches of Le Roux, Jamin and Roger, that the resistances determined from the application of Ohm's formulæ to the conditions in the experiment do not correspond at all to the real resistance of the generator in repose, and that the latter should be augmented in a large proportion. (See the "Exposé des applications de l'électricité" of M. du Moncel, vol.ii, p. 271.)

It is then necessary to determine these resistances directly, so as to be able to state precisely what causes are in operation, and from them to deduct practical conclusions, and it is this which Mr. G. Cabanellas has just done in his first work which he has submitted to the Academy of Sciences. The difficulty was to avoid the induction produced by various causes which develop electro-motive forces in the wire of the

ring, rendering it impossible to measure by the Wheatstone Bridge method. The most important of these accidental inductions is that which results from the action of terrestrial magnetism, which is capable by itself alone of generating currents relatively energetic, as was shown by the experiments of M. Lami. To remove this, M. Cabanellas has opposed to one another the effects produced by it on two similar Gramme rings, moving in the same conditions, and in this manner he could determine without the intervention of that cause of disturbance the measure of the resistance of the double ring, submitted to a movement of rotation. Whether the ring was moving or in repose M. Cabanellas could make it the fourth side of a Wheatstone Bridge, and in consequence he could measure the resistance in both cases. The following are the results which he has obtained:

The resistance of the moving ring, with respect to that of the same ring maintained in a fixed position and for a mean rate of rotation of 450 turns per minute, showed an increase of about 25 per cent. (the temperature of the two rings being the same), and this increase was not at all modified by the intervention of metallic resistance introduced between the two connected rings, or outside of them. The variations produced in the electro-motive force of the generator (within the limits which allow the use of the Bridge of the Siemens Universal Galvanometer) appear to have no more influence on the measurements whether the rings were in motion or repose, and the influence of weights applied to the collectors of the rings produced differences only of a secondary order. With powerful electro-motive forces, it has been the same, but M. Cabanellas has employed for verification an arrangement of two galvanometers, one of low resistance placed in the circuit, and one of great resistance placed in a shunt, which has enabled him to obtain direct measurements, without necessitating the use of standard resistances specially made of large wire. The resistance of each ring was then given by the proportion $\frac{E}{Q\,I}$.

It would have been interesting to study the manner by which the increase of resistance varies with the speed of rotation of the rings, so as to fix the laws of the phenomena, but the experiments made at the establishment of Messrs. Sautter, Lemonnier & Co. required such great derangements of the business that M. Cabanellas, not willing to abuse good nature, has postponed the further experiments which he intends to make.—*La Lumière Electrique.*

THE THEORY OF THE TELEPHONE.

Our readers will remember that in 1879 M. Ader constructed a telephone without a diaphram, the sound being given out by a soft iron wire enclosed in a helix, through which the current passed. The action was explained by du Moncel as due to longitudinal vibrations in the wire resulting from successive lengthenings and shortenings of the wire under the action of the magnetizing helix, it being well known that an iron core is lengthened by magnetization. M. du Moncel employed this fact to support his molecular theory of the action of the Bell telephone. Since that time M. Boudet, of Paris, has, by means of his sensitive microphone, succeeded in revealing these changes of length. Moreover, M. Ader himself has investigated the subject very closely, and besides measuring the effect in question has arrived at the important principle that *all bars of magnetic nature submitted to a mechanical action, be it compression, torsion or tension, tend to recover their primitive molecular arrangement under the influence of the current which magnetizes them.*

The apparatus employed by M. Ader in his experiments consists of a small straight electro-magnet, having its core formed of an iron wire 0·04 or 0·05 metres long, and one extremity chamfered off. Upon this chamfer acts a long lever, which serves the double purpose of an indicator and exciter of the mechanical action. The lever terminates in an index which moves itself before a graduated scale (in front of which is a lens) and is connected to the magnetic core in a manner differing with the mechanical action to which the latter is submitted.

In the cases where compression and tension are produced two distinct effects are seen. On allowing the current to flow for several instants continuously the index is gently displaced, moving up the scale in the former case (compression) and down the scale in the latter case (tension), with a deflection proportional to the intensity of the battery and the time of the closure of the current. This effect is evidently due to the dilation of the core in each case under the influence of the heat developed by the current, for it is obtained with a core of copper. On closing the circuit momentarily the lever rises instantly in both cases, and instantly falls on the current being interrupted. At the same time a "dry" sound can be perceived at each make and break of the cur-

rent when the ear is applied against the table supporting the apparatus. But the apparatus is so arranged that the displacements indicated in the two cases correspond to two mechanical effects diametrically opposite in character—that is to say, to a lengthening and shortening of the core. It can, therefore, only be due to a molecular action tending to annul the mechanical strain on the core in either case.

In the case of torsion, the index rises always at the moment of making the circuit and falls again at the moment of breaking it, and in a degree corresponding to the force of the mechanical action, as can be determined by a weight sliding on the torsion lever. In this case the thermal effect is not shown at all, since the indicating lever does not change its position by reason of the lengthening or shortening of the magnetic needle. The magnetization in this case tends, as in the experiments of MM. Guillemin and Coulon, to untwist the wire.

The length by which the core is shortened by closing the circuit momentarily by the current, according to M. Ader's calculation, is less than one ten-millionth of a metre, or one ten-thousandth of a millimetre. It is greater under the influence of torsion, and less under the influence of tension.—*The Electrician.*

Mechanical Theory of Heat.—In Jacob Hermann's *Phoronomia sive de Viribus et Motibus Corporum solidorum et fluidorum, libri duo*, 1716, sections 658 and 659 treat of the internal movement of fluids and of heat as dependent upon molecular motion. The latter section contains a demonstration of the theorem that "heat, other things being equal, is in a ratio compounded of the density of the heated body and of the duplicate ratio of the disturbance of each one of its particles. *Ann. d. Phys. u. Chem.* C.

Occlusion of Gases in Aluminium and Magnesium.—M. Dumas finds that not only iron and silver possess the property of retaining large amounts of gas for an indefinite period, but that aluminium and magnesium also have the same property. For equal weights magnesium contains a volume twice as great as aluminium. But magnesium is so much lighter than aluminium that it will hold only one and a half times its own volume. Silver seems to prefer oxygen; aluminium and magnesium hydrogen, each metal appearing to have its individual preference.—*Comptes Rendus.* C.

New Magnetical Experiments.—M. Ader has performed some interesting experiments upon bodies which are slightly magnetic. He finds that elder pith is more sensitive than ordinary wood, paper, etc. With a Jamin magnet, sustaining a weight of 100 k. (220·46 lbs.), and provided with two small polar armatures, separated by an interval of 2 mm. (·78 in.), he has been able to attract, at a distance of 3 mm. (·118 in.), a pith ball of 5 mm. (02 in.), suspended by a thread. He has been able even to raise it, and when once attached to the magnet considerable force is necessary in order to remove it.—*Comptes Rendus.* C.

Discovery of a New Elementary Body in the Lava of Vesuvius.—Prof. Scacchi, the celebrated mineralogist and crystallographer, in studying the yellow incrustation which is found on certain portions of the Vesuvian lava ejected in 1631, and which he calls Vesbine, has discovered a body which differs in its character from all the known elements. He considers Vesbine as a vesbiate of alumina, and he regards vesbic acid as the oxacid of a new element, *vesbium*, which, according to the properties of its compounds, will probably stand between molybdenum and vanadium. Stas thinks that vesbium may be found in some of the common ores, where its presence has never been suspected because it has been confounded with some of the known acidifiable metals.—*Bull. de l'Acad. Belg.* C.

Influence of Light, Copse and Mould on Forest Growth.—M. Gurnaud has conducted a series of observations, during the past nineteen years, upon the solid contents of annual growth in a forest among the Jura mountains. He finds that: 1. Light, when it strikes the ground without having been sifted by foliage, stimulates the production of carbonic acid in the decompositions which are engendered by the humus, together with a decomposition of the same gas by the chlorophyll. 2. The growth of wood diminishes when the underbrush intercepts, to any great extent, the access of light to the soil and diminishes its reflex action on the branches of the trees. 3. This interruption of reflex action is due mainly to the spread of the underbrush, for when it is kept trimmed the vertical shoots which are retained interpose no obstacle. 4. The mould, under too great a thickness, loses a part of its virtue, and, like barn-yard manure which is too deeply buried, remains inert for many years.—*Comptes Rendus.* C.

Bronzing Liquid.—Dissolve 10 parts aniline red and 5 parts aniline purple in 100 parts of 95 per cent. alcohol, in a water-bath; then add 5 parts benzoic acid, and heat the mixture until the greenish color changes to a light bronze-brown. This liquid, when brushed upon metal, leather or wood, produces a beautiful bronze effect.—*Bayer. Ind. u. Gewerbeblatt.* C.

Precaution against Fire-damp.—The connection of mining explosions with low barometric pressures has long been noticed. In the colliery of Seraing barometers have been lately placed near each of the ventilators, with directions to give the ventilating fans 75 turns per minute in fine weather, 80 turns in changeable weather, and 85 turns in stormy weather.—*L'Echo Indust.* C.

Alcohol in Animal Tissues.—M. J. Bechamp finds that alcohol may be detected in a large variety of animal tissues, even during life, as well as after death, in such quantities as to throw great doubt upon the evidences of alcoholic poisoning which have hitherto been regarded as conclusive. While agreeing with Pasteur, in most of his views as to the nature of ferments, he thinks that the bacteria which appear in the putrefaction or other alterations of animal tissues do not come from atmospheric germs, and he recounts a number of experiments which seem to confirm his views.—*Am. de Chim. et de Phys.* C.

Utility of Solar Boilers.—M. Abel Pifre describes a variety of modifications of Mouchot's apparatus, adapted both to domestic and to industrial use. He thinks that they are of especial importance in connection with the projected railway to Central Africa. The Barbary figs, which are very abundant, yield about 25 per cent. of alcohol, more than the sugar beets. A single solar boiler, with a reflector of five metres diameter, can distil, without cost for fuel, 200 litres (52·8 gal.) of this alcohol per day, the raw material costing nothing, and the refuse being valuable for the food of cattle. The importance of this result becomes evident upon considering that Algiers imports every year more than 30,000 hectolitres (792,500 gals.) of alcohol, and that it will need to import still more for the manufacture of its wines, which require the addition of alcohol in order to bear transportation. Pifre is now experimenting upon the conversion of solar heat into electricity, and of the electricity into mechanical work, by the intervention of a Gramme machine.—*Mem. de la Soc. des Ing. Civ.* C.

Phosphorescent Lighting. — Dr. Phipson takes sulphide of barium, or some other substance which is rendered phosphorescent by the solar rays, and encloses it in a Geissler tube, through which he passes a constant electric current of a feeble but regular intensity. He claims to obtain in this manner a uniform and agreeable light, at a cost lower than that of gas.—*Les Mondes.* C.

Transparent Gold.—If a solution of gold in aqua regia is neutralized by carbonate of soda, and a solution of oxalic acid is added, the gold is precipitated in a brilliant yellow powder. On examining this precipitate by the microscope the flakes are found to have a triangular or hexagonal form and to be translucent, the color of the transmitted light depending on the thickness of the crystals.—*Les Mondes.* C.

Transplanting Eels.—The French engineering corps has been experimenting for some years past upon the transport of young eels by railway, in order to supply various streams in the different departments. Very little care is necessary except to avoid shipment when the temperature descends below the freezing point. They have already successfully transplanted about 1,500,000 eels.—*Ann. des Ponts et Chaus.* C.

Planetary Arrangement.—In recent papers before the French Academy MM. Gaussin, Faye, Chase and Schötel have discussed various indications of law in the arrangement of the planets and some of their bearings upon the nebular hypotheses of Laplace and Hershel. The indications which were given by Prof. Chase were the closest and most striking, furnishing numerical evidence of a projectile force connecting the solar system with the fixed stars.—*Comptes Rendus.* C.

Influence of Light on Size of Leaves.—M. Ch. Flahault, in the *Annales des Sciences*, brings forward additional observations to support his view that under equal conditions the leaves of plants of the same species are larger in proportion as we go nothward, these relatively higher dimensions being due to the duration of light of relatively feeble intensity. In cases where the chlorophyll is formed in the absence of light, it must be formed at the expense of the materials stored up in the tissues. The importance of these reservoirs of nutriment is still greater in the case of flowers. Thus, of hyacinths, both red and blue, M. Flahault found no difference in the color of the flowers grown in the light or in the dark, the color being manufactured from the stores of material in the bulbs.